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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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REVIEW: 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.

AUTHORS: We would like to thank the reviewer for his/her comments. We attempt to answer to the above comments below.

REVIEW: 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 result in n 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.

AUTHORS: A number of different approaches exist to describe the dielectric properties of frozen hydrometeors assuming that frozen hydrometeors are made up of ice inclusions in an air matrix. Their dielectric properties can be combined in a number of different approaches, i.e., Bohren and Battan (1982), Fabry and Zsyrmer (1999), or Maxwell Garnett (1904). Using these different methods, among others, the authors have found very little difference to the simulated TBs, and to the authors knowledge those proposed in Petty and Huang (2010) would not either. The authors, however, acknowledge that the symmetry in the Bruggeman formula with respect to the inclusions/hosts provides a more realistic approach for treating particle densities ranging progressively from solid ice to mostly air, as there is no need to make an arbitrary decision as to when the ice switches from being the matrix to being the inclusion, as mentioned in Petty and Huang (2010).

REVIEW: 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, 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.

AUTHORS: The authors acknowledge that a frequency dependent density introduces some sort of tuning parameter that is physically unrealistic, and that the next step is, the authors agree, to study the impact of using the Liu et al. (2008) DDA database. However, the availability of the database has allowed a number of similar studies that have shown that it is easy to improve results in one situation but worsen them in others because the problem remains as to how to choose an appropriate particle shape(s) e.g., Geer et al. (2014). The particle sizes and shapes are poorly known and are subject to large variability. Kulie et al. (2010) for example looked at constraining the choice of particle shape from the DDA database using a radar and passive microwave observations, but due to the large variability in hydrometeor habits, no recommendation

was concluded. In this context, the Liu et al. (2004) approximation was a pragmatic approach to as it is based on the realistic DDA database. We are now working directly with the DDA data base, but other problems occur, such as the compatibility between the assumption to calculate the DDA data basis and the particle parameterization in the cloud model outputs (such as the density).

REVIEW: 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, meltwater 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).

AUTHORS: A similar comment was made by the other reviewer about the description. A better description of the assumptions is included. Particularly here, the authors focus on the comments made above:

* The dielectric constants used for water and ice are described in Section 3.3, making it clear that Liebe et al. (1991) and Matzler (2006) are used respectively. This is again emphasized in Section 4.1. Note that no fixed temperature for ice and water are used in the dielectric constants, the temperature is imposed by the cloud model outputs (Meso-NH for the given layer). * More on dielectric constants, shapes and PSD: Line 385 (Section 4.1) reads: “The ïňĄrst step in the radiative transfer simulations is to stay as consistent as possible with Meso-NH. In order to do this, the microphysical description of hydrometeors from Meso-NH is ïňĄrst used. The mass-size relationships and the particle size distributions described in Section 2.1 are adopted for the 5 species

provided by Meso-NH (rain, cloud, ice, snow and graupel) and all hydrometeors are considered spherical.". We have added the following: "The dielectric properties of rain, liquid cloud and ice are described in Section 3.3. For snow and graupel species, the dielectric properties are calculated using the Maxwell-Garnett mixing formula for dry snow with an ice volume fraction f = (snow)/(ice), where (snow) is the density of snow and graupel as deduced from the mass-relationships.". * Surface property assumptions for TB calculations are determined from TELSEM and FASTEM emissivity tools, which are coupled with ARTS as described in Section 3.1 * For the clarification asked about "meltwater 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)" ARTS is fed with individual profiles for the 5 different species that Meso-NH outputs, each described by its own single scattering properties and PSD. This means no continuity, other than that modelled by Meso-NH, is forced onto the RT inputs to describe the melting process or the melting layer i.e. the authors do not explicitly model a melting layer, as it is not included in Meso-NH, as in most cloud models. * Cloud liquid water attenuation/emission: this also came up on the other review and there is hardly any liquid water in the core region leading to significant attenuation.

REVIEW: 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.

AUTHORS: This is true. The following has been added at the end of Section 3.1: "The microwave simulations are obtained at the model grid resolution at the direction of the center of the antenna pattern; they are then directly compared to the coincident passive satellite observations". In reference to CloudSat, the following is added in Section 3.2: "The radar simulations are calculated in reference to the CloudSat altitude bin resolution (125 vertical bins 240 m thick)."

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