

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-73

Anonymous Referee #1

Referee comment on "Bulk hydrometeor optical properties for microwave and sub-millimetre radiative transfer in RTTOV-SCATT v13.0" by Alan J. Geer et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-73-RC1>, 2021

Mixture of technical and scientific comments:

eq 18 is weird: $\hat{\sigma}(\sigma(D_g)/m(D_g) \times m(D_g)$ -- looks like the $m(D_g)$ terms would cancel out?

Radar definition: Lines 855-860: "Here, by definition of the reflectivity factor, the backscatter cross-section is assumed to be given by the Rayleigh scattering

approximation ($\hat{\sigma}(\sigma(D_g) = z_0 D_g^6$)" This simplification shouldn't be necessary, except in that limiting case where it applies. From the particle properties, you have all of the backscattering information necessary to compute the non-Rayleigh reflectivity. Furthermore, for ice-phase hydrometeors, one would have to consider the modifications to the dielectric constant in the "z0" term, unless you're explicitly making the decision that all reflectivity factors are computed *as if* the reflecting hydrometeors are liquid water with an equivalent diameter.

In figure 1, what are the effective diameters of the assumed size distributions for the various hydrometeor types given the fixed water content? Similar comment for Fig. 2, particularly for consideration of the radar reflectivity, which is strongly dependent on the size of the particle.

Lines 274-275: "In Fig. 1 the frozen particles have small oscillations with frequency, particularly obvious in the radar reflectivity at lower frequencies. This is a result of interpolating away from the original temperature, size and frequency steps in the ARTS database." This seems more like an error in the interpolation routine. In every other study I've seen that provides similar plots, this behavior is not observed on interpolated points.

Equation 6: Is D_g equivalent to D_{max} for non-spherical particles, and if how is this handled in the size-integration over various D_{max} values, while maintaining the appropriate mass-dimension relationship?

With regard to this as well, it wasn't clear to me (perhaps I missed it), but an accounting of the difference in density between bulk ice and liquid water need be made if one is using liquid-equivalent diameter as the baseline for all mass comparisons across hydrometeors. See, for example: Bennartz, R. and Petty, G.W., 2001. The sensitivity of microwave remote sensing observations of precipitation to ice particle size distributions. *Journal of Applied Meteorology*, 40(3), pp.345-364.

Line 302, units for density are incorrect / inconsistent.

Eq 8 is the $\rho = \rho_0$ intentional? Also, may want to mention that this is the "k-th moment" equation, and in later equations, " $b = k$ ".

Table 4: SI units?

Section 3.3.2: You can use the incomplete gamma function solution to definite integrals, this is discussed in Petty and Huang 2011. Not a recommended change, just an observation.

Line 538, water content is typically expressed in $k \text{ m}^{-3}$? See e.g., Line 651. Perhaps "water path" would be a more appropriate term when referring to the vertically integrated content (even if for a single layer).

Lines 550 area: There's some hand-wringing about D_g as the diameter variable of choice, but I think much effort would be saved using D_e instead. The logic is that D_e is a proxy for mass, by virtue of being the mass equivalent radius. This ensures equivalent mass comparisons in the PSDs, and greatly simplifies phase transitions such as melting / freezing. This is, however, not a recommendation for this paper, as it would require a complete overhaul of everything done so far.

Line 665: "this warming effect" is simply due to the fact that smaller particles are more emissive than scattering at higher frequencies.

Line 669: "Above this frequency of maximum scattering, these clouds start emitting more radiation again and brightness 670 temperatures are higher." True, but it's also relevant

to point out that surface-induced polarization effects (e.g., over ocean) ALSO decrease with increasing optical depth -- this provides a separate key piece of information content.

Overall, very well written paper, and I have no substantial requests for revision beyond what might be suggested from the above comments.