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# Comment on amt-2021-216

Anonymous Referee #3

Referee comment on "Retrievals of ice microphysical properties using dual-wavelength polarimetric radar observations during stratiform precipitation events" by Eleni Tetoni et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-216-RC2, 2021

**Review:** "Retrievals of ice microphysics using dual-wavelength polarimetric radar observations during stratiform precipitation events"

by Tetoni et al

**General comments:** This is a tough paper to review. On one hand the measurement setup and collected dataset is very interesting. On the other hand, there are several things that causes concerns.

I understand the problem the authors are facing and that is behind one of the weak points in the study. The authors are using the "soft spheroid" particle model in combination with T-matrix to compute single-scattering ice particle properties that are used for the retrievals. However, given a large number of articles published in the last decade that argue that such approach could lead to significant errors, this argument is not easy to make. It is not impossible, since one could argue that 35 GHz measurements are not suffering from the "non spheroidal" effect that much. However, the authors tried to avoid the existing literature and use not well supported arguments, see detailed comments below. I suggest that they improve this part of the paper. Please use more up-to-date literature and make your arguments using the current understanding of the problem.

The second problem is related to how reliable the retrieved values are. Because of the measurement setup the radar observations volumes are mismatched. Potentially because of this, the observed DWR and retrieved Dm values show artificial looking patterns. The authors use the retrievals to generate statistics of particle properties, see Fig 12, which is one of the main stated goals of the study. I would like to see a discussion what retrieved values can be trusted and why. Ideally, problematic data should be excluded.

Finally, there are statements in the manuscript that are not correct. A good example is the definition of the reflectivity factor. These should be corrected.

Overall, in my opinion the manuscript needs significant improvements before it can be considered for the publication. I encourage the authors to revise it significantly and resubmit.

**Specific comments**:

Line 30 ” Ice clouds can cause a cooling effect at the surface by reflecting the shortwave, incoming solar radiation but they can also contribute to warming of the atmosphere by trapping the longwave, terrestrial radiation (Liou, 1986). ”

Are you sure about this statement? To my knowledge, ice clouds have a net warming effect. It is possible that in some particular cases they would lead to cooling, but the warming effect is more common.

Line 49-50: “Another way to gain microphysics information is to use multi-frequency radar observations as they exploit the scattering properties of ice particles in both Rayleigh and Mie regime.”

Strictly speaking, Mie regime is not a correct term, a better one is the resonance scattering regime or non-Rayleigh scattering. The Mie solution is only applicable to spherical droplets.

Equation 1a – This equation is derived assuming small (much smaller than the wavelength) spherical water droplets.

Line 64-65 “Similarly, the equivalent radar reflectivity factor Ze can be calculated when the radar reflectivity η is measured as well as the dielectric factor of water |ð□□¾|2 = 0.93 and Rayleigh scattering is assumed: ”

This is incorrect. Please check the definition of the radar reflectivity factor and equivalent reflectivity. Also, what is η, you have not defined it.

Line 96, Straka’s paper is not presenting a method to distinguish hydrometeor types but summarizes characteristics of hydrometeors and corresponding dual-polarization radar signatures. There are many papers that actually present the method.

Line 107 "Snow hydrometeors with size in the millimeters order of magnitude are found to have low densities and therefore, the Self-Similar Rayleigh-Gans Approximation (SSRGA, e.g. Hogan and Westbrook, 2014; Hogan et al., 2017; Leinonen et al., 2018a), that is applicable for "soft spheres", can be used. "

Actually, the exact opposite is argued in (Hogan et al., 2017; Leinonen et al., 2018a) and many other studies i.e. by Kneifel et al. (2015). SSRGA is introduced exactly because we cannot use the "soft sphere or spheroid" approximation. SSRGA takes into account internal distribution of ice particle mass that in its turn affect RGA's form-factor and therefore the scattering properties. "Soft-sphere or spheroid" approximation assumes that mass is uniformly distributed, and inclusions are much smaller than the wavelength and hence an effective media approximation, such as Maxwell Garnett, can be used. These approximations seem to fail at mm-wavelengths.

Line 110: " A well-proven approach is the soft spheroid particle model which uses the effective medium approximation (EMA) to model the refractive index of ice crystals and aggregates, e.g. the Bruggeman or Maxwell-Garnett models as in Garnett and Larmor (1904). "

What are the assumptions and for which conditions EMA are proven to work? This is a blank statement implying that EMA always work regardless of conditions.

Line 112 : "Many studies, e.g. Hogan et al. (2012), have …"

There are many studies after that which argue the opposite. As I have mentioned above the development of SSRGA was motivated by the inability of a "soft spheroid" particle model to reproduce the observations. You should use more recent literature to argue the point and make more convincing argument.

Line 122 and up to line 116 "Moreover, using spheroids we can better understand the ambiguities between the aforementioned degrees of freedom. Here, more sophisticated models of specific ice crystals could introduce additional challenges to sort a collection of ice shapes along these degrees of freedom or to define variables like the aspect ratio."

The aspect ratio and orientation angle are part of the "soft-spheroid" particle model. These parameters are irrelevant for a more complex particle representation. One should keep this in mind, while interpreting observations and using different particle models for such interpretations. If you have selected “soft-spheroid” as your model of a more complex ice particle, then you should expect that your model is a (possibly over) simplified representation. Whether this is an advantage or not, it is a matter of discussion. So please make a stronger argument of your point?

**Aspect ratio**, page 8: Please use commonly used definitions of AR. At the moment, it is very confusing.

Line 276 “Furthermore, small oscillations out of this plane with a standard deviation of 20° are included to consider the flutter of ice crystals. ”

How? Please explain what you mean.

Line 304, equation 6: What was the motivation of using the melted snowflake diameter?

Page 11. Look-up table structure

Given that you are using the “soft spheroid” particle model and T-matrix to compute single-scattering ice particle properties, I was expecting a discussion on how the refractive index is defined for different AR values. Are you preserving particle mass or density (and therefore the refractive index)? This should be explicitly discussed.

What are PSD integration limits used in calculations? How did you select the maximum D value? The selection of maximum D has a direct influence on Zdr. Is that the reason why you are having an issue with reproducing Zdr observations?

Fig 11, page 15 How physical are high Dm values closer to the cloud top? Please explain what data can be trusted. Ideally you should mask questionable data. This affects the results presented in Fig. 12.

Page 17. Unknown mass-size relationship

Please explain, what was the logic behind the selection of the m(D) relations. Are they representative of the events you have observed, i.e. represent particles observed in this temperature regime? Are they sufficiently different to cover a possible range of values? On lines 290 -300 you just state that you use them without much discussion why.

“The BF95 mass-size relation was found to model to low ice particle densities … BF95 prescribes near-zero density values for large particles. This leads to very low simulated ZDR values with increasing particles size (Fig. 6)”

Hogan et al (2012) was able to reproduce observed Zdr values. Could you please explain what is the difference between their and your study?

Fig. 16 page 20. I think this is the most interesting plot of the paper. I am not sure how practical this is, but the difference between a and b panels indicates that there is extra information that can be retrieved by using different measurement geometries. Very interesting.