Manuscript Review
Cairo et al.

The study that is described in this manuscript purports to evaluate methods for computing light scattering from aspherical particles using the T-matrix technique. The approach is to use PSDs measured in situ with an OPC and to calculate backscattering coefficients and depolarization ratios that are then compared to backscattering and depolarization measured directly with remote sensing techniques. It is my opinion that this manuscript was submitted prematurely and I recommend that the authors withdraw this submission and resubmit once they have addressed a number of issues that I list below. My decision to reject this paper is because the issues that I raise will require more than just a major revision and the paper has little to offer in its present form that can provide the scientific community with information that is useful in understanding PSCs or that can assist in interpreting measurements from in situ or remote sensors.

In their introduction, the authors state: *"The aim of our study is to employ concomitant microphysical and optical measurements of PSCs when both liquid and solid particles are present and compare these with optical scattering computations done with codes capable of reproducing depolarization in backscattering. This allows us to verify the capacities and limits of these codes, tested on a relevant data set which provides both microphysical and optical measurements of mixed-phase particulate distributions. The methodology illustrated is not restricted to the study of mixed phase PSCs, but can find applications in all those cases in which the aerosol appears as an external mixture of solid and liquid particles, distinguishable on the basis of their different typical sizes."*

By their own admission, they were unable to verify the capacities and limits of the code, not because of any flaws in the code itself, but most like due to how they applied it under very simplistic assumptions. I would add that I think that how they applied the code was also too simplistic, e.g. assuming that all the particles had the same AR rather than trying combinations of size-dependent AR.

Again, in my opinion, the concluding statement that *"However, the result for the backscatter coefficient simulation allows to constrain the model parameters Rth and AR into reasonable ranges and should be regarded as the positive result of the study"* does not justify publishing this study in its current form. The reason for this opinion is because the authors never explain how their study will benefit the science, not in the introduction nor in the concluding remarks; hence, the questions are 1) What purpose does constraining the model parameters serve?, 2) On what basis do they declare that these ranges are reasonable? and 3) Since they conclude that using prolate and oblate spheroids to model the scattering did not lead to useful results, that constraining the AR range is meaningless.

I think that for this study to be published and provide useful information to the broader scientific community, it must do the following:

1) Explain the importance of knowing the particle sizes and shapes in mixed phase PSCs.
2) Diagnose their model results to understand why they are producing unacceptable comparisons.
3) Run a sensitivity analysis using simulated PSCs and measurements to quantify the observed difference.
4) Correct a large number of typographical and grammatical errors that made the current manuscript distractive to read.

Other comments, questions and suggestions:

1) From the abstract onward the authors erroneously talk about comparing "microphysical and optical" measurements. This makes no sense since all of the measurements are microphysical and optical, i.e. the OPC uses an optical technique to derive size distributions that help describe the microphysical properties of the PSCs. Likewise, the remote sensing techniques are optical and are also used to derive microphysical properties of PSCs.
2) The authors never explain the relative importance of mixture of particle types in mixed phase PSCs. Had the modeling exercise been successful, who would benefit?
3) Nothing is discussed about the contribution to the backscattering of other types of stratospheric particles, e.g., meteoritic dust, sulfate particles, etc. How does that impact the measurements and modeling?
4) The backscatter instrument described by Adriani (1999) had multiple wavelengths. Why is only the 532 being used? Wouldn't modeling multiple wavelengths have improved the retrievals?
5) If this was a true modeling study, an iterative methodology should have been used to vary the mixtures of shapes and sizes until most closely matched by the measurements.
6) How homogeneous are these clouds and what do the PSDs look like derived from the OPC? The reader never sees the actual shapes of PSD or what the number concentrations are. This is important because it will impact the backscattering and depolarization. It is stated in the results section that apparently the larger particles are biasing the depolarization but this depends on the total concentration of particles and how homogeneous the mixture is. I could not find in the Adriani (1999) paper what beam volume is at each measurement gate.
7) In Figs. 4 and 5, there is no noticeable difference between AR-0.5 and AR=1.5. This does not surprise me because if you have an ensemble of randomly oriented spheroid, an oblate spheroid will look like a prolate spheroid, depending on their relative orientations; hence why even use ARs < 1?
8) There is no quantification of the comparisons, i.e. no correlation coefficients, curve fits or other statistical tests applied to justify comments like "fine" or "reasonable. In fact, the authors' conclusions that the backscattering comparison is "fine", does not agree with what we see in the figures where the dispersion is hidden by the logarithmic scales on the figures.
9) I recommend that the analysis of the OPC data to derive backscattering should use the actual scattering measured by the OPC, rather than converting scattering to

equivalent optical diameters and then computing scattering. This adds additional uncertainty because there are large errors in size derivation because of Mie oscillations and unknown shape. If the authors derived backscatter from the measured forward scattering, as was done by Baumgardner and Clark (1998), this removes much of the inherent error.

10) I was disappointed by the excessive typographical and grammatical errors since the second author is a native English speaker. *"Author contributions. FC was responsible for most of the writing,* ***review and editing process, supported by all co-authors.****"* This appears to be inaccurate.

Reference

Baumgardner, D. and A. Clarke, 1998: Changes in aerosol properties with relative humidity in the remote southern hemisphere marine boundary layer, J. Geophys. Res., 103,16525-16534.