Further comment on egusphere-2025-3065

The Authors have attempted to refute the concerns of all three reviewers about potential bias in the retrieved values of the asymmetry parameter g. However, their arguments are insufficient, or in some cases plainly wrong. For instance, they claim in the response to Reviewer 2 that "the circular-aperture approximation introduces only negligible error into the asymmetry parameter". This is simply not true. To show this, I have calculated the asymmetry parameter for synthetic phase functions (broadly resembling the analytic phase function due to Baran et al. (2001)) constructed using diffraction on distributions of randomly oriented ellipses with mean equal area-equivalent size parameter of 101 and aspect ratios of 4 and 1 − i.e. the latter were circular. The resulting g values are 0.735 and 0.754, respectively. Therefore in this case the circular aperture assumption introduces a discrepancy of +0.019, see Fig. 1. For size parameter 300, well into the "geometric optics" regime, the difference is still +0.01. In general, non-circular apertures shift the phase function towards larger angles, reducing g. Note that these errors alone are an order of magnitude or more larger than the value claimed in the original manuscript (0.001). Yet accordingly to the manuscript the diffraction calculation contributes one half of the retrieved asymmetry parameter (since PHIPS does not measure in this angular region), hence this is a serious weakness. For this reason I fail to see why "the circular-aperture approximation introduces only negligible error into the asymmetry parameter" because it concerns only the low scattering angle region that PHIPS does not measure: on the contrary, the lack of measured data in this region means that the retrieval relies at low angles solely on the diffraction calculation!
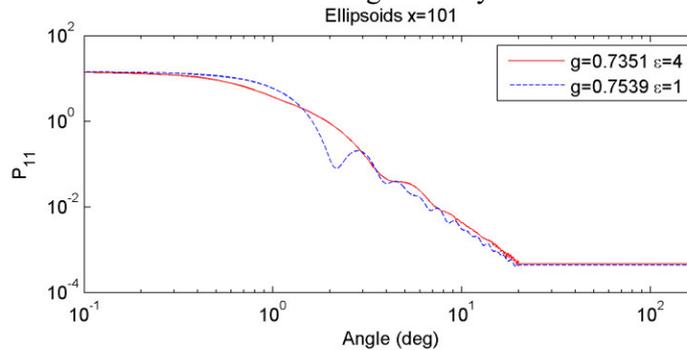

Figure 1. Synthetic scattering phase functions constructed using diffraction on randomly-aligned elliptical aperture distributions with equivalent mean size parameter of 101 and aspect ratios of 4 and 1 (circles). The resulting values of g are given in the legend.

Some other responses by the Authors show characteristics of circular argument. It is claimed for example that using truncated Legendre polynomial expansions is justified because "our measurements do not contain smooth planar crystals". This by implications means that the retrievals rely on the absence of "smooth" crystals, and as a consequence cannot produce high g values. Yet in situ measurements in cirrus find a variety of crystal geometries, including smooth ones.

The Authors justify the choice of old techniques (pure GO) by their widespread use in the past and the existence of a large body of literature. Yet using such techniques after they have been supplanted by improved, more accurate and more physically grounded techniques (e.g. IGO, see Yang et al. (2019) for a summary) is unwise. Is applying Newtonian mechanics to orbital calculations justified now because it was in wide use before the development of general relativity?

Yet at the same time I note that the revised details provided in the response to Reviewer 3, the g uncertainty is 0.02, contradicting the Authors' vehement denial of potentially large g measurement error in their response to Reviewer 1, where they insisted that the errors were below 0.001. It is worth considering here the impact of a 0.02 bias in g. Using the Authors' Eq. 8 for a simplistic cirrus case, reflected TOA SW flux might change by some 5 W m^{-2} even for thin cirrus, hardly a negligible amount. Yet the bias could be even larger, considering that the reviewers have identified several separate potential sources of error.

It is worth remembering that relative accuracy is typically easier to achieve than absolute one. This is reflected in the very weak difference found by the Authors between Arctic and mid-latitude cirrus. However, systematic bias is a different matter. The Author's claim of large reduction of g with

respect to previous assumptions must therefore be sufficiently well justified - strong claims require equally strong evidence. This requirement has not been met here.

I note that Reviewer 3 proposed a clear way to test the accuracy of the technique that was used by the Authors – which I too strongly recommend - and which would have provided the needed evidence. However, the Authors have chosen not to take advantage of it. Instead, they insist on relying a computational technique that is known to be inaccurate and/or have been superseded by better ones.

Therefore I cannot support the publication of this work as it stands.

References

Baran, A. J., Francis, P. N., Labonnote, L.-C., and Doutriaux-Boucher, M.: A scattering phase function for ice cloud: Tests of applicability using aircraft and satellite multi-angle multi-wavelength radiance measurements of cirrus, Q. J. Roy. Meteorol. Soc. 127, 2395–2416, https://doi.org/10.1002/qj.49712757711, 2001.

Yang, P., Ding, J., Panetta, R. L., Liou, K. N., Kattawar, G. W. and Mishchenko, M.: On the convergence of numerical computations for both exact and approximate solutions for electromagnetic scattering by nonspherical dielectric particles, Progress In Electromagnetics Research, 164, 27–61, https://doi.org/10.2528/PIER18112810, 2019.