I appreciate the effort that the authors have put in to address my extensive comments. I have one significant comment warranting major revisions.

The definition of albedo susceptibility used in the text is ambiguous as it depends entirely on the vertical reference level used to construct the spatial variation of the albedo. As touched on in my previous comments, when the altitude of the TOA albedo is a distance from the cloud top that is much larger than the horizontal domain size, the albedo will become spatially uniform. This is an immediate geometric consequence of the cosine weight in the definition of irradiance. Then, as the variance of the albedo tends to zero, the regression slope, β, of horizontally-varying albedo against Nd (or any other variable) will tend to zero, following from the definition of the regression slope:

$$\beta^2 = \frac{r^2 Var(N_d)^2 Var(\alpha)^2}{Var(N_d)^2},$$

where r is the pearson correlation coefficient. Since the reference level is a geometric convention, it could be chosen in an arbitrary way to select a smaller or larger albedo variance, and hence a smaller or larger albedo susceptibility. With multiple cloud fields, if the variance of albedo in each domain is chosen to be near zero, the regression will simply be of domain mean albedo vs. Nd. This is obviously an indeterminate concept of albedo susceptibility as the correlation between Nd and the heterogeneity of each cloud field can be chosen to produce a desired apparent albedo susceptibility.

This present study used a reference level of 3 km. Another study could use the exact same cloud fields (i.e., a medium with participating media in the same volume only up to 3 km) but use a TOA altitude of 20 km and get a significantly different result (i.e., one closer to zero). It is also possible to re-register the radiance field to arbitrary altitudes (including those below the domain top) or to some surface. As I noted in my comments from the previous review, there is already an operational approach to register irradiance to a height surface derived from stereo matches. So, the many possible conventions are not esoteric issues, they are already well-established practices. Why not evaluate the spatial variability of albedo at the TOA, when it is the susceptibility of TOA albedo to cloud properties that is ostensibly being estimated?

Each of these conventions will produce a different covariance between droplet concentration and albedo. None of them are correct. None of them are the “truth”, i.e., consistent with classical physics. They are unvalidated approximations.

Regardless of the geometric convention used to construct the horizontal variability of albedo, the real albedo susceptibility is unchanged. The sensitivity of area-averaged TOA albedo to a particular column's optical properties is invariant to the height of the domain top and to the geometric convention. A consistent differential form for the 3DRT preserves this property. The present study's definition does not. This is because, in fact, there is no unique height surface on which to register fluxes to associate them with the column-by-column optical properties.

The authors have argued that the implementation of a calculation of albedo susceptibility using 3DRT is out of scope of a study that focuses on the calculation of albedo susceptibility using 3DRT. This does not make sense to me, personally. While the portions of this paper covering droplet concentration retrievals are valuable, I have yet to be convinced that this study estimates the "Impacts of three-dimensional radiative transfer effects on [...] albedo susceptibility."

In lieu of making changes to the numerical results, my request to the authors is simple; **please explain everything in your article**: Introduce the exact calculation of albedo susceptibility, introduce your approximation, and discuss the implications. Then, justify your interpretation of the results to the reader without skirting around the textbook definition of irradiance in 3D geometry.

I'm insisting on this change because I can already envision a future in which others perform similar analysis without considering 3D geometry, leading to confusion about the albedo susceptibility is, and its variation with cloud type. The readers of this study deserve a clear explanation of what is being calculated vs. what is claimed to be calculated. At the moment, I would not consider the response to the reviewers stating that a "truth" for albedo susceptibility is now being reported in the article to be accurate. By including the required explanations, this paper can head off this confusion rather than contributing to it, thereby providing a more lasting contribution.

Additionally, this doesn't require action in the present study but I mention it as a heads up for the future. The integration scheme for the flux is numerically unstable in 3D grids. Using radiances every 22 degrees in zenith results in long-characteristic calculations with sparse support. This leads to "ray effects" as the distance between the position the radiance is evaluated at and the position of variability in the source function (i.e., distance between domain top and cloud top), increases. A "ray effect" is when the rays along which the radiance are calculated miss most of the variability in the source function while integrating. This means that the flux at the TOA will have more spatial variability than the truth, and their derivatives with respect to column properties (e.g., calculated using finite differences or a numerically consistent adjoint calculation) will be inaccurate. Currently, this issue is mitigated as the distance between flux calculation and cloud-top is small, but it will not generalize well. In numerical models such as SHDOM, the default flux integration is performed with short characteristics which have numerical diffusion that compensates the error caused by the ray effects. This is another reason to use a forward-adjoint framework with an existing 3D RT solver, as a numerically stable flux calculation in 3D is non-trivial.