The authors present a methodology for estimating aerosol radii (mode radius of the lognormal distribution) using a single wavelength combination (510 nm and 869 nm) from the OMPS-LP record. The authors introduce this method and provide a comparison with balloon-borne optical particle counter data as well as a coincident SAGE III/ISS profile as validation of the technique. Having demonstrated the validity of this technique the authors then apply it to data collected after major disruptions (i.e., the 2019 Raikoke eruption and the 2022 Hunga Tonga eruption).

Major Concerns with this Manuscript

I have two major concerns about this publication:

1. The general methodology is not new. A very similar method was published in 1983 by Glenn Yue (DOI: 10.1364/AO.22.001639) and another method (using 2 ratios) was published in 2021 by Felix Wrana (DOI: 10.5194/amt-14-2345-2021)). Both of these papers (and numerous others in between) deal with extinction ratios from SAGE instruments, but the gist is the same.

2. The methodology itself is fundamentally flawed and the derived products are wholly unreliable. I present a simple model below to demonstrate this unreliability. The authors assumed that the information content of 1 extinction ratio is sufficient to derive a valid estimate of particle size, but this only holds true if the distribution width is fixed and the measurement error is sufficiently small; both are invalid assumptions. While an assumed distribution width of 1.6 is a good estimate, fixing the width to that value (or any other value) imposes an artificial constraint on the solution space and inevitably biases the inferred radii and number density results. Ultimately we have to recognize that we know very little about the atmosphere (the width could be 1.2, or it could be 1.9; both are very realistic) and forcing the distribution width to 1 specific value is wrong.

Given the flawed methodology and the lack of novelty it is difficult to see how this paper makes a substantive contribution to the scientific literature (we can get the same information from a paper written 40 years ago). However, I recognize that I may have missed some nuances of their method so I ask for clarification on several points.

Specific Comments

1. A comment on the references: The authors cited many manuscripts that do not correspond to the text they supposedly support. For example, on lines 54–56 the authors state that their method of determining particle size is based on 4 previous publications and all of the cited papers deal with cloud identification and filtering, not determination of size. Further, the Bourassa et al. 2007 paper does not seem to fit at all. The same is true for the Bourassa 2014 paper cited on line 73. Bourassa 2014 has to deal with stratospheric ozone trends. Perhaps the authors intended to cite Bourassa 2008 instead, but even that paper does not support their text (Bourassa 2008 cites Deshler et al. 2003, but the context within which Bourassa 2008 is cited here indicates that they actually did in situ measurements, which they did not).

2. Line 10: The authors claim that they demonstrate that extinction ratio is insensitive to aerosol concentration. This is nothing new and can be observed by looking at the corresponding equations.

3. Line 51: As stated in this paper the OMPS-LP retrieval assumes a size distribution to obtain the extinction products. The authors then used the extinction products to infer a size distribution, which makes a cyclical process. What if the assumed size distribution used in the OMPS-LP processing was different, would that change the derived size? What is the level of correlation between the assumed size distribution and the derived particle size?

4. Line 60 (all of section 2): It is unclear whether the authors accounted for the uncertainty in the OMPS-LP products. Given the content of some of their figures I assume they did, but it is never explicitly stated (see comment below regarding error propagation).

5. Line 72: It is unclear why the authors assumed a distribution width of 1.6. Granted, this value makes for a reasonable first guess, it used in SASKTRAN, and was used by Bourassa et al 2008 (the authors cited Bourassa 2014). However, Deshler et al. 2003 in no way claims that 1.6 is the only value that should be used. The Deshler et al. 2003 paper presents a figure (Fig. 5 panel B) that contains a derived size distribution from 1 altitude (20 km) of 1 profile; this in no way supports the use of a static distribution width of 1.6. This is a key point.

    (a) The authors took this value of 1.6 (collected during the "background period" at 20 km), failed to account for the natural variability of this value and made the assumption that it never changes. This is particularly a problem when the authors use the same distribution width after major eruptions.

    (b) The distribution width in the atmosphere is not static. It changes with season, altitude, latitude. The width is highly variable even when the atmosphere is not substantially impacted by volcanic and/or pyroCb activity (see Fig. 1). While the assumed width of 1.6 may be reasonable, it cannot be assumed to be static.

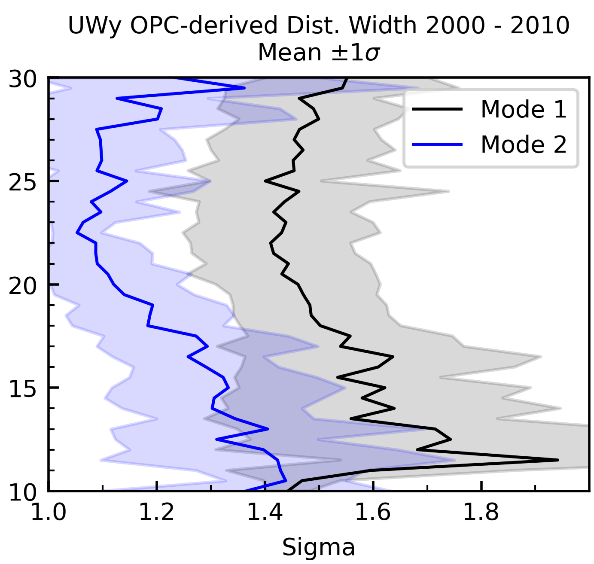

Figure 1: Profile of mean (± 1-sigma) distribution widths from the University of Wyoming OPC record. Data collected during the quiescent period (2000–2010).

(c) The University of Wyoming dataset reports an uncertainty in distribution width of ±20%. Therefore, even if Deshler et al. 2003 said that the width is consistently 1.6 that would still leave a range between 1.56 and 1.63. If we make some assumptions we can model the expected behavior: assume sigma error is fixed at 20% (per Deshler et al. 2003) and the measurement error (propagated uncertainty in the extinction ratio) is *only* 5% (this is conservative as Taha et al. 2021 report accuracy/precision on the order of ±20%). Here (Fig. 2) we see the range of mode radii that produce extinction ratios that fall within these uncertainty bounds (everything from ≈40 nm through 190 nm). The question then is "Which mode radius is the 'real' one, or which do you pick?" Each radius is a viable solution so the uncertainty in the authors' estimate is far larger than they show.

If the authors were to use a realistic uncertainty in their estimate of distribution width (e.g., along the lines of the atmospheric variability shown in Fig. 1) and were to account for the propagated measurement uncertainty then they would see the solution space expand quite rapidly. This point cannot be overstated: the distribution width is highly important and is far from static, fixing the width to 1.6 (or any other value) imposes an unjustified constraint on the solution space and introduces bias in the inferred radius estimates as well as the corresponding number density estimate. It is for this reason that I see the method as fundamentally flawed.

The model I present in Fig. 2 is overly conservative and presents a best-case scenario. The point I'm getting to is: Even under these best-case scenarios we cannot make a definitive statement about the particle size. The requisite information content is not there.

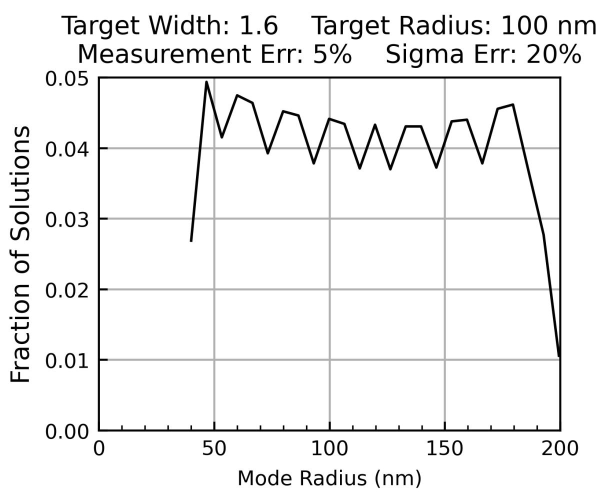

Figure 2: Distribution of radius solutions when the error in distribution width is fixed at ±20% and the uncertainty in extinction ratio is fixed at ±5%.

6. Line 78: The authors suggest that the "CR" is "only a function of size" and I am uncertain of what is meant by that. The CR is a function of particle size distribution parameters (both mode radius and distribution width).

7. Line 79: Could the authors please explain why the 510/869 combination was chosen instead of 510/997? The 510/997 combination would expand the "usable" range from 0.4 μm to $\approx$0.5μm.

8. Lines 80–81: "This CR – size relationship allows us to infer the median aerosol particle radius up to $\approx$0.4 μm." This will vary depending on the distribution width.

9. Lines 99–100: "Thus, if we use the L2 AE at two wavelengths, we have enough information to independently compute a size and number density..." This is not true as demonstrated above. Even with multiple extinction ratios you would not have enough information to definitively determine particle size. The best we can do is report a range of radii.

10. Lines 103–104: In the previous paragraph the authors stated that their method was "independent of the radiative transfer model assumptions", but now they state this is a potential source of error. Could the authors please clarify?

11. Lines 129–130: The authors stated "The uncertainty ranges of OMPS-LP retrievals are calculated from the extinction coefficients (AE), using the formula below". The context of this paragraph led me to believe that Eq. 2 was used to calculate the error in derived mode radius...but this is just an error propagation. Could you authors please clarify how this equation was used to generate the errors in their Fig. 3 & 4?

12. Section 3.2: The purpose of this section is unclear. I see 2 possibilities:

    (a) Do the authors present this as corroboration of their size estimate? If so, then this fails as all this demonstrates is that the SAGE III/ISS extinction ratios are in agreement with those of OMPS-LP.

    (b) Do the authors present this as validating the OMP-LP extinction ratios (i.e., since the derived radii agree with the radii derived from SAGE III/ISS then OMPS-LP and SAGE III/ISS must be reporting the same extinction ratios)? I wonder because later in this section the authors state "The agreement validates our assertion that errors due to Mie phase function variation with size are minor and that the extinction estimates from the OMPS- LP L2 algorithm are robust." (lines 173–175). If this was their intent, then why is this needed and why does this fall within this paper (it seems a stark departure from the stated intent)? Also, didn't Taha et al. 2021 already do this validation?

I thank the authors for taking the time to read my comments and look forward to their feedback.