Responses to one of the community reviewer's comments (CC5)

We appreciate the feedback from all three community reviewers, who are all members of the SAGE III/ISS team. Their comments predominantly imply that SAGE III/ISS is the only satellite instrument capable of providing stratospheric aerosol particle size distribution (PSD) data. While we recognize the critical role of SAGE III/ISS, this perspective overlooks the need for alternative sources of PSD data beyond its operational lifetime, which is expected to conclude with the decommissioning of the ISS in 2030.

Additionally, the reviewers do not specifically address the PSD results presented in this paper, which align well with published literature despite larger uncertainties, inherent to the measurements.

The OMPS LP instrument stands out as a promising (if not the only) candidate for continuing PSD observations in the coming decades. This paper represents the first effort to leverage OMPS LP's multi-wavelength capabilities to derive PSD, a contribution we hope will encourage further refinements, both by our team and the broader community, to address the anticipated data gap once SAGE III is no longer operational or lacks coverage.

The following are our point-by-point responses to the community CC5, with his comments copied and pasted in italics.

Overview

The authors present a revised version of a paper that was originally submitted in 2023 (https://amt.copernicus.org/preprints/amt-2023-36/). Unfortunately the current incarnation of this manuscript fails to provide substantive changes from the previous submission and the authors fail to address any of the major challenges that were raised. To date, the methodology they present remains fundamentally flawed (this claim is supported by figures that the authors provide in their manuscript and will be expounded below), and the authors continue to gloss over these critical aspects without warning the reader of the impact of their assumptions. Rather, the authors "sup- port" their assumptions by incorrectly citing various papers, ignoring other key papers that refute their assumptions, and ignoring the plethora of in situ data that invalidates their assumptions. An obvious case of this last point can be observed in their Fig. 10. If the authors were to plot the OPC-measured distribution width (herein referred to as either "distribution width" or σ) they would show the reader the large degree of variability σ has within a single profile (for this flight σ varied from 1.18 to 2.45). This plot alone would be enough to tell the reader that a static distribution width is not only ill advised, but wrong. Instead, this point is ignored throughout the text and the reader is left with an incorrect impression of the integrity of this method.

Response: We appreciate your detailed feedback from the community reviewer. We understand that your primary concern is with our use of a static distribution width (σ) in our methodology. We have addressed this issue by performing a comprehensive uncertainty analysis, as presented in Section 2. This analysis was introduced in response to recommendations from earlier reviewers.

We acknowledge that some references were incorrectly cited in the initial submission. This was unintentional, and we have corrected all the errors highlighted by the reviewers. If there are any additional references you believe are still incorrect, we would appreciate it if you could point them out, and we will address them accordingly.

Regarding Figure 10, we would like to clarify that our study focuses on retrieving the aerosol particle median radius rather than the distribution width (σ). While we recognize the importance of the variability in σ, it falls outside the scope of our current study to compare this parameter directly with in-situ measurements. However, we are planning to address this aspect in a future study, which will specifically investigate the variability of σ under different stratospheric conditions.

We agree that a static distribution width introduces greater uncertainties compared to a dynamic approach. We have included a detailed explanation in the manuscript discussing our rationale for assuming a static distribution width for the OMPS-LP data. The main reason is the limited spectral range of the OMPS-LP measurements, which makes it challenging to distinguish variations in σ accurately.

This is a major problem with this method: using a static distribution width inevitably forces the rm to a specific subset of the solution space which may, or may not, be close to the actual atmospheric conditions. The problem is, given this method, we will never know whether the assumed σ value was right or wrong (and, by extension, we will have no confidence in the inferred rm estimate) unless we happen to be fortunate enough to have a coincident OPC flight that can be used to inform the authors' algorithm and confirm the output (if the authors were to provide a rigorous validation of their algorithm then we could certainly have more confidence). The authors demonstrate this by shifting their σ value from 1.6 to 1.2 for the HTHH event; how would they know to do this without the OPC data? Further, the σ in that profile changed, substantially, throughout the profile (again, see their Fig. 10). Shifting their assumed σ from 1.6 to 1.2 made it a better guess in parts of the profile, but a far worse guess in other parts of the profile (again, without the OPC data, and in the absence of a robust validation effort, how would we know where the assumed σ is "good" or "bad"?). In short, even when we have OPC data to inform our assumptions we must be very careful on how these assumptions are used because the atmosphere, especially after eruptions and/or wildfires, is not homogeneous.

Regarding the static distribution width, one may wonder how the authors justify their value (σ =1.6). They repeatedly, and incorrectly, refer to the Wrana et al. 2021 (https://doi.org/10. 5194/amt-14-2345-2021) paper. I can only speculate how they misinterpret that paper (perhaps it is a misinterpretation of the Wrana et al. 2021, Fig. 4, which is for a single profile). The problem is that Wrana et al., 2021 did not provide a statistical evaluation of the distribution of σ values (or any other size distribution parameter). However, Wrana et al. did state "The upper and lower boundaries of the colour bars in Fig. 6 roughly mark the ranges within which the values of the respective quantities fall for the data set between June 2017 and December 2019. The median values for this time frame at 20 km altitude are 130.6 nm for the median radius, 1.54 for the mode width σ,..." Granted, the median σ value, at 20 km, from Wrana et al. 2021 was 1.54 (close to the authors' value of 1.6), but the authors must recognize that the color bar scale for σ (Fig. 6 of Wrana et al. 2021) extended from ≈1.1 to 2.0. This is a direct refutation to the claim that Wrana et al. 2021 supports the selection of a static σ of any value.

The authors make no mention of the bias in the OMPS extinction products under aerosol loading as describe in Bourassa et al., 2023 (https://doi.org/10.1029/2022GL101978), the so-called 1-D assumption and convergence problems. Bourassa et al. 2023 showed that, when the stratosphere is volcanically perturbed, the extinction is a factor of 2 too high at the aerosol peak, while, below the peak, the extinction product is a factor of 2 too low. While Bourassa et al. evaluated the performance of the 755 nm channel the conclusion of the paper is clear: the bias is inherent in the overall retrieval methodology and there is no reason to believe this bias is limited to a single channel (we note that the University of Saskatchewan's tomographic retrieval method alleviates, but does not obviate, this bias). Work done in our group (unpublished, but Mahesh Kovilakam suggested that he will provide figures in his review of this paper) indicates that this bias is not constant across wavelengths, but increases as you move to shorter wavelengths; hence, the bias does not cancel out in the ratio.

Finally, the authors fail to provide a convincing validation of their new product, which is essential. They provide a comparison with 2 balloon-based in situ measurements and declare both to be in "reasonable agreement" (line 281). However, the agreement in their Fig. 10, within the main aerosol layer, was off by at least a factor of 3 (i.e., the OPC measured rm remained steady at ≈50 nm from 16–21 km, while the OMPS method yielded rm estimates that ranged from ≈150 to >300 nm) and the profile shapes are not even close to being similar. If anything, this "validation" exercise proved the opposite of what they intended (i.e., it showed quite clearly that their product is not reliable outside background conditions). Can the authors justify calling this "reasonable agreement".

Responses: The first point has to do with available information in the OMPS extinction measurements. These issues are summarized in Knepp et al. (2024). Basically, there is insufficient information to accurately determine the concentration as well as characterize the PSD mode radius and distribution width (σ) with the OMPS wavelengths. This is fair point. Wrana et al. (2021, 2023) and Duchamp et al. (2024) retrieve a median radius and log-normal distribution width using two SAGE color ratios, the second employing the 1.543 extinction. Because of the narrower OMPS wavelength range, we cannot independently retrieve both r_m and s.

To estimate the log-normal size distribution width Knepp et al. (2024) takes a different approach from Wrana et al. (2022). Knepp varies log normal size distributions creating a solution space. Then they extract and equivalent radius and distribution width from the color ratios from the observed extinctions. This approach also yields a measure of uncertainty. We downloaded Knepp's files and generated a histogram of distribution widths for data between 45N and 45S between 20-30 km and before 2022 (to avoid Hunga). The normalized distribution of sigma values is shown below, the vertical line is the mean value.

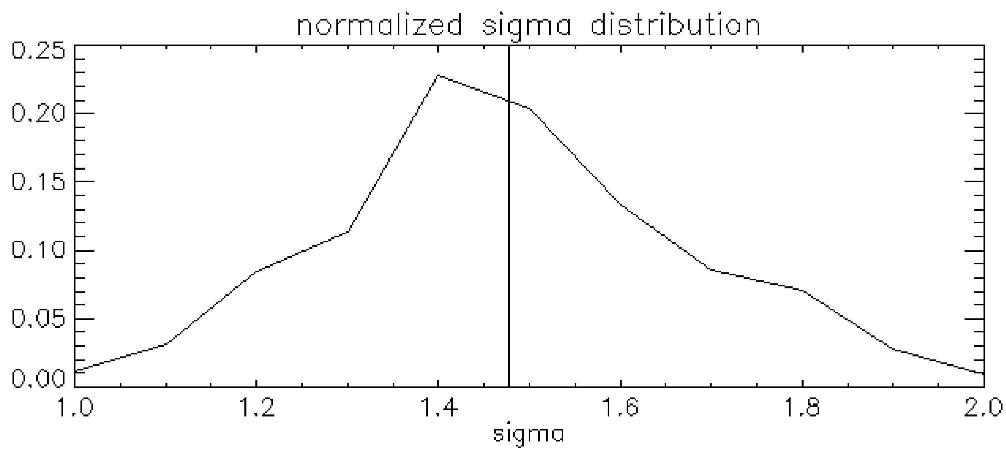

Figure 1. The normalized distribution of sigma values (distribution widths) for data obtained from Knepp et al. (2024) within the latitude range of 45°N to 45°S and altitude range of 20-30 km, prior to 2022 (to exclude the Hunga event). The vertical line represents the mean value.

Our approach is related to Knepp et al. (2024) in that we are trying to estimate the error by varying the distribution width. We first assume that we can extract r_m from the color ratio. As pointed out by CC3 and CC8, there is difference in the OMPS extinction compared to SAGE. This uncertainty is reported in Taha et al. (2021) and noted in our text. CC8 argues that we should expand the discussion of the errors and we have done so.

The Knepp et al (2024) retrievals suggest that assuming 1.6 probably widens the distribution too much, and, we agree with the reviewer that a universal value is hardly justified. Under ambient conditions there is plenty of observational data suggesting that a width value of 1.6 is reasonable (see Wrana et al., 2021, Fig. 6 and discussion in Reiger et al. 2014; Table 1). The figure (above) we generated from Knepp's files supports a slightly smaller value for σ, but 1.6 is not unreasonable. We certainly agree that under volcanic conditions, however, the width may be much smaller as determined by Duchamp et al. (2023). Using SAGE data, Duchamp retrieved Hunga particles with r_m =0.35μm. This agrees with our results using OMPS when we adjust σ - our Fig 14. Duchamp used the method developed by Wrana et al. (2021).

Validation of the OMPS product is difficult since few balloon measurements intercept OMPS locations exactly. Aside from criticizing the lack of coincidences, the reviewer doesn't provide alternatives to validating the data. A comparison with Knepp's analysis is beyond the scope of this current work, but we have started to make comparisons with Knepp's analysis (see above), and that will be the subject for a future paper.

The authors go on to provide what they call an "evaluation" of their product by running OMPS and SAGE III/ISS data through their algorithm to determine how the derived rm values differed (e.g., their Figs. 7 and 8). What they fail to recognize is that the agreement is foreordained from the outset and that any agreement they show here is really a convoluted comparison of the 2 instruments' extinction ratios. This in no way provides an evaluation of their algorithm. If the input numbers are similar then it is impossible for the output to differ.

The authors miss an opportunity to compare their product with the recently release SAGE III/ISS particle size distribution (PSD) parameters (Knepp et al., 2024; https://doi.org/10. 5194/amt-17-2025-2024). I do not fault them for this as our product was only made available within the last few months. Further, I do not suggest this as a shameless self promotion of our work, but the SAGE PSD parameters provide a unique data set that provide uncertainty estimates for the PSD parameters, all of which could be used to evaluate the performance of their algorithm. Unfortunately, we already know how their algorithm will perform since we did a similar evaluation of deriving PSD parameters from SAGE II (i.e., using a single extinction ratio of 520:1020 nm). This is shown in section 6.3 of Knepp et al. 2024 with the statistics summarized in Table 9 of that paper. What we observed is the rm estimates were off by up to -124%, -95%, -28%, 3%, and 21% at the 10th, 25th, 50th, 75th, and 90th percentiles. In short, 80% of our estimates were between 124% too low to 21% too high. There simply is not enough information within 2 channels (i.e., a single extinction ratio) to make an accurate estimation of rm.

Responses: In response to the strong opinions expressed in your above comments and all community comments that all from your SAGE III/ISS team, we have decided to remove Section 3.1, which compared of OMPS-LP PSD retrievals with SAGE III/ISS. Whereas we did make it clear that comparison to SAGE was not validation but algorithm verification, CC5 and other community reviewers also misconstrued the point. Since other readers will likely also be confused we removed the section.

Finally, all papers require a purpose, the "so what" factor. After reading it, we should be able to answer the question "What did this paper tell us that is new?" or "How is the community now smarter?" The authors fail to deliver on this point.

Responses: This an opinion and not an actual comment on the methodology or results in the paper, but we will address these questions further below.

Herein, the authors put a new dataset (OMPS color ratios) through an aerosol size algorithm that was developed more than 40 years ago and this methodology is as limited in information content now as it was then.

Responses: We are well aware that color ratio has been used before and added references as such. However, overall application of this technique to the OMPS-LP data set has not been done before. This is new research. Also new is the sensitivity of the retrievals to assumptions about the distribution widths.

What did we learn? The method is not new and we do not gain any new insights into the atmosphere. Since the method is not new I would expect, at the bare minimum, a thorough demonstration of the validity of this method (i.e., a robust validation), but instead we get 2 comparisons with OPC profiles and an ill conceived evaluation with the SAGE III/ISS instrument.

Responses: As we noted above the comparison with SAGE was verification of the algorithm not validation, and we clearly stated that in the text. However, we have removed that section. There is little truly independent validation data available, and we have made use of what we have. Note that SAGE II aerosols were validated by OPC predecessor instruments (Wang et al.,1989) using a similar approach and just seven coincidences. Specific to the validation shown in the paper, the comparisons with balloon-borne OPC measurements are crucial, especially since they include analyses under two different conditions, ambient background and post-volcanic eruption scenarios. These comparisons provide a good understanding of the algorithm's performance across varying atmospheric conditions. While the validation is not exhaustive, we believe it is a meaningful demonstration of the method's capability and lays the groundwork for further validation studies.

If the authors were to make this product available to the community it could not be used without the end users first doing the validation work that should have been done here.

Responses: Here was an opportunity for the CC5 to point us at additional validation material that he would recommend – and yet ...

In my view there is no scientific merit in this work.

Responses: As stated, this is a 'view' and not a fact. We strongly disagree.

Returning to the reviewer's overview comments.

"What did this paper tell us that is new?"

The paper applies the color ratio algorithm to OMPS data which has not been done before. The paper shows the uncertainty in assumptions about the size distribution reflect on the uncertainty in the retrievals.

"How is the community now smarter?"

The community is now smarter in that users should apply color ration algorithms cautiously to OMPS measurements especially during extreme events. Indirectly, we show that OMPS wavelengths are insufficient to retrieve both rm and s .

The reviewer clearly recognizes that the two-wavelength retrieval algorithm has a long history and is still widely used in the scientific community because of its robustness and applicability in retrieving aerosol properties from satellite observations.

Our study demonstrates why this well-established algorithm is particularly suited to the OMPS-LP instrument.

Applying this conventional particle size retrieval algorithm to the OMPS-LP is indeed significant. It highlights OMPS-LP's capability to provide valuable information on stratospheric aerosols, which is crucial for monitoring long-term changes in aerosol loading and understanding their impact on climate and atmospheric chemistry.

Moreover, we have conducted a detailed uncertainty analysis that introduces a novel approach to quantify uncertainties in aerosol size retrievals. This analysis considers the uncertainties arising from measurement errors, the assumed distribution width, and phase function assumptions. We believe this contributes new insights into the limitations and potential of aerosol size retrievals from satellite observations.

We are committed to making this dataset available to the community with clear documentation on its limitations and intended applications.

Brief synopsis of the authors' work

Herein, the authors demonstrate how the size (radius, herein referred to as rm) of stratospheric aerosol can be estimated using extinction ratios (or color ratios, herein referred to as CR) using OMPS data. This method is not novel, and the authors appropriately cite the Yue and Deepak (1983) paper. This method is based on finding CR values in a Mie theory lookup table that fall within the expected uncertainties. From there, the authors average the radii of particles that fall within their solution space and return an estimated rm. The authors then compare their estimates with those measured on 2 flights of the University of Wyoming's Optical Particle Counter (OPC) and then apply their method to OMPS data collected after 2 volcanic eruptions: Raikoke and HTHH. The authors also present a sensitivity study to determine the impact of different OMPS viewing geometries on their rm estimates.

General Concerns with the Manuscript

– This paper requires a thorough proof reading to correct the multitude of grammatical errors, incomplete sentences, and passages that are difficult to interpret.

– The authors make claims throughout the manuscript and provide references to bolster these claims. Many of the cited works do not support these claims. Some of these papers have nothing to do with what the authors are claiming. This is particularly egregious because many of these issues were raised in the first round of reviews for their original manuscript, yet they remain unchanged here. Please check all references and confirm they are correct.

Responses: We sincerely apologize for any grammatical errors and unclear passages in the manuscript. We have conducted a thorough proofreading and have corrected all identified issues to ensure clarity and readability throughout the text.

Regarding the references, we acknowledge that some were incorrectly cited in the previous version. We have carefully reviewed and updated all citations to ensure they accurately support our claims. If there are specific references you believe still do not align with the statements made in the manuscript, we would appreciate further guidance so that we can make the necessary corrections.

– The authors cite other works that demonstrate a fundamental misunderstanding of the cited work. The Wrana et al. 2021 paper is particularly abused within this manuscript. These issues were raised during the review of the original paper and, to date, remain unchanged. This should be corrected before resubmission.

Responses: We appreciate your feedback regarding the use of Wrana et al. (2021) in our manuscript. As we replied above, we cited this work specifically to support our assumption that a distribution width of 1.6 is appropriate under ambient conditions, as indicated in their Figure 6 and corroborated by balloon measurements. For scenarios involving volcanic eruptions, we have referenced Duchamp et al. (2023), which provides relevant information on distribution width under these conditions.

We understand the importance of accurately representing cited works and have carefully reviewed our citations to ensure they are used correctly. If there are specific aspects of Wrana et al. (2021) that you believe have been misinterpreted, we would appreciate further clarification so we can address this concern comprehensively.

– The authors gloss over glaring issues within the OMPS data product and how these problems will impact their rm estimates. Bourassa et al., 2023 (doi.org10.10292022GL101978) and Gorkavyi et al., 2021 (doi.org10.5194amt-14-7545-2021) discuss the bias in the OMPS extinction product, but the authors never address this issue herein. I have discussed this with Mahesh Kovilakam, who will also submit a review that goes into more detail on this aspect, but this can be easily seen by comparing the global half-to-1-micron ratio of OMPS to that of SAGE.

– Previous work (Bourassaetal.,2007oi:10.1029/2006JD008079) demonstrated that Rayleigh optical depths, for scattering instruments, are high enough at lower wavelengths (i.e., 510 nm) that limb scattering observations are insensitive to aerosol signatures. This relates back to the previous comment. Perhaps this is a misunderstanding on my part, but after having numerous conversations about this paper (conversations that involved people outside our working group and outside our organization) I can confidently state that lack of fidelity in the OMPS 510 nm channel is a common perception (a perception that is supported by anal- ysis involving the 510 channel). It would greatly benefit the reader if the authors were to demonstrate that the 510 nm channel is reliable and refute the claims of Bourassa et al. 2007.

Responses: We acknowledge the concerns regarding the quality of the OMPS data product and its potential impact on our retrievals of aerosol properties. While the detailed evaluation of OMPS data biases is beyond the primary scope of our study, we have used the OMPS aerosol extinction coefficient product as provided and focused on deriving aerosol properties based on this dataset.

We understand the significance of addressing these issues, as highlighted by Bourassa et al. (2023) and Gorkavyi et al. (2021) and the sensitivity of the 510 nm channel by Bourassa et al. (2007). In our response to Comment CC8, we have discussed these data quality concerns in more detail.

Specific comment on this manuscript

The authors original statements will be presented in quotation marks (black font), while my comments and questions will be in blue.

1. lines 12–14: "We apply our algorithm to extinction coefficient measurements made by the Stratospheric Aerosol and Gas Experiment on the International Space Station (SAGE III/ISS) to verify our approach and find that our results are in good agreement."
Whether you call this a "verification", "validation", or "evaluation", it does not matter be- cause it was not done here. There was no validation/evaluation/verification of the algorithm. The

authors used OMPS data to estimate rm and then used a coincident SAGE profile to es- timate rm. This provides nothing more than a roundabout comparison of the two extinction products for a few select profiles (none taken under volcanic aerosol loading, which avoids the issue pointed out by Bourassa et al., 2023). The authors should either perform a meaningful validation of their product or remove this claim from the manuscript.

Responses: The whole section related to SAGE III/ISS is deleted in this revision.

2. line 36: Reference to Thomason et al., 2021

This is an incorrect citation. This paper did not estimate particle sizes as the authors claim (other than to say the particles were "probably big" or "probably small"). This reference should be removed and all citations should be checked to ensure they are appropriate throughout.

Responses: Deleted.

3. line 55: Here the authors provide a brief enumeration of uncertainty sources in their products. Reference to the "arch effect" as discussed in Bourassa et al. 2023 and Gorkavyi et al. 2021 would be appropriate here as would the Rayleigh scattering optical depth issues as raised by Bourassa et al. 2007.

Responses: added.

4. line 60: "Our algorithm is similar to the color ratio method developed by Thomason and Vernier's (2013)..."
As written, this sounds like Thomason and Vernier 2013 determined particle sizes, which they did not. Please clarify.

Responses: Modified.

5. line 75: It seems odd to refer to Q as scattering efficiency here since you are dealing with extinction efficiencies. This seems like a unnecessary detour. Please consider whether this nomenclature is necessary (your call on whether it is changed or not).

6. line 76 and Eq. 1a: The PSD equation is a function of r (array of particle radii over which Eq. 1a is integrated), rm, and σ. Please update for clarity.

Responses: Updated.

7. line 76: Why the jump from Eq. 1a to Eq. 3?

Responses: It is better to show the log-normal distribution details in Section 2.2.

8. line 77: It would be helpful to the reader to define N, r0, and s here.

Responses: Updated.

9. line 80: "For occultation measurements p = 1 and Eq. (1a) is the same as Wrana et al. (2021) Eq. 2."
It is unclear what the authors are trying to communicate here. As written, their Eq. 1a is identical to Wrana's Eq. 2, which makes this statement tautological. Please clarify.

Responses: Deleted.

10. Line 121 in regards to Fig. 2:

Though not critical to the paper, why the discrepancy at -8.21/-78.98? Why would the OMPS retrieval stop >10 km above the cloud top?

Responses: That profile is the closest match to the balloon measurement shown in Figure 9. The retrieval issue you mentioned is outside the scope of this study.

11. Lines 146–148: "Wrana et al. (2021) used a third extinction wavelength from SAGE III/ISS data to estimate the log-normal distribution width and found that most of the observations clustered between s = 1.4 and 1.6. This information also constrains our size distribution uncertainty."

The authors misinterpreted this paper. Wrana et al. 2021 does not support this assumption. Here, the authors ignore a wealth of data that refutes the assumed range of distribution widths. For example,

(a) the Wyoming OPC record shows a lot more variability especially outside background conditions.

(b) Wrana et al. 2021 (Fig. 6) showed far more variability, between 2017–2019, than the authors claim. From Wrana et al. 2021 "The upper and lower boundaries of the colour bars in Fig. 6 roughly mark the ranges within which the values of the respective quantities fall for the data set between June 2017 and December 2019." It is important to note that their colorbars extend from ≈1.1 to 2.0, which refutes the authors claim.

(c) Wrana et al. 2023 showed more variability than what is claimed here.

(d) Knepp et al., 2024 (https://doi.org/10.5194/amt-17-2025-2024) likewise showed

more variability. This product is readily available for download and use.

Ultimately, this results in the authors imposing an artificial constraint within their algorithm that preferentially steers their algorithm to a subset of solution spaces.

12. Lines 151–152: "The uncertainty in CR, UCR, can be estimated from Taha et al. (2021, Fig. 6) comparison to SAGE III/ISS." As written, I do not understand what this sentence means. Please revise.

*13.	Line 156: "Rieger et al. (2018, Fig. 6) which gives a width uncertainty of ≈0.2"*
The authors misunderstand Fig. 6 of Rieger et al. 2018. This figure tells the reader the range of PSD values used in their simulations and is descriptive, not prescriptive. The authors are not using Rieger's Fig. 6 for its intended purpose and this figure does not support their σ uncertainty.

*14.	Line 156: "...consistent with the Wrana et al. (2021) estimate."*

Again, the Wrana et al. 2021 paper does not support this claim.

Responses: For comments 11 to 14, please refer to our earlier response regarding the distribution width.

*15.	Line 159: I do not understand what is meant by "outside of the averaging domain." Would the authors please clarify?*

Responses: We mean Points located outside the averaging domain depicted in Fig. 3 are excluded.

*16.	Lines 160–161: "...common distribution widths (s=1.4–1.6)."*

Defining this range of distribution widths as "common" is heretofore unsupported.

Responses: Please refer to the response above for the main comment.

*17.	Lines 163–170: I apologize, but I do not understand what the purpose of this paragraph is or how it fits within the context of the surrounding text. Would the authors please check that this should be here and does not need revised?*

Responses: This paragraph provides further explanation of Figs. 3 and 4 and includes an example to demonstrate the application of the figures.

*18.	Lines 169–170: "This example shows how the radius and number density uncertainty due to the distribution width can be quantified."*
There is no doubt that uncertainty can be quantified, the fact remains that these values are calculated incorrectly. The range of σ values has been artificially constrained to yield an artificially small uncertainty. If the authors were to use a reasonable range of values then their uncertainty estimates would be so large as to make their estimate of rm meaningless (especially under enhanced aerosol loading).

Responses: We do not agree that the range of σ values has been artificially constrained to produce a smaller uncertainty. In fact, the range of σ is from 1.1 to 1.8, as shown in Figure 4, which is comparable to the range in Wrana et al. (2021), from 1.1 to 2.

*19.	Line 173: The jump from Fig. 4 to 11 here is confusing. Please number figures and equations sequentially, in the order in which they appear in the text as this will aid the reader.*

Responses: Figure 9 is more appropriate in Section 4, where aerosol properties under different conditions are discussed. Mentioning Figure 9 here is optional and simply serves to provide readers with an example to help understand our concept. The current Section 2.2 primarily focuses on evaluating retrieval uncertainties.

20. Lines 174–175, in reference to Fig. 4.

The content of this figure is misleading. As discussed above, the authors used an artificially small (and unjustified) uncertainty in σ. Second, the authors neglect to account for the bias inherent in the NASA OMPS retrieval algorithm (as discussed by Bourassa et al. 2023). Failing to account for these issues misleads the reader and skews your subsequent analy- sis and conclusions that are based on this figure. Please recreate this figure using correct uncertainties and accounting for bias under elevated aerosol loads.

Responses: Please refer to the response above for the comment 18.

21. Section 2.3

Here, the authors fail to account (or mention) the bias inherent in their retrieval as discussed by Bourassa et al. 2023. If the authors want this to be applicable to volcanic conditions, then this issue must be addressed.

22. Lines 202–207 Here, the authors seem to conflate bias and measurement uncertainty. What they show in Fig. 6(c) is a bias in their CR (an offset) that is caused by assuming an incorrect size distribution. Because this is an offset it cannot be used in place of an uncertainty in their error propagation. Please correct.

23. Line 206: "...we get an uncertainty range of 0.15–0.3 μm"
What is the range if the authors correctly calculate their errors (i.e., using realistic values of σ)?

Responses: For comments 21 to 23, please refer to the response above for the main comment.

24. Section 3.1

This section is unnecessary. The authors state "The Fig. 7 comparisons verify our assertion that errors due to aerosol phase function variation with radius are minor (see Section 2.3), and that the extinction coefficient estimates from the OMPS-LP L2 algorithm are robust." However, they previously stated that previous studies have evaluated the OMPS/SAGE extinction products so it is difficult for me to see the value of performing yet another com- parison, much less one that is based on derived products (products that involve more as- sumptions and an additional level of abstraction). The purpose of this paper is to introduce a new method for inferring particle sizes from OMPS extinction coefficients and this section does not fit within the scope of that purpose. This in no way provides a validation of their method and cannot, under any condition, be interpreted as a validation or even a meaningful evaluation.

Despite how good Fig. 8 may look, it is ultimately meaningless because, again, you are not comparing 2 independent estimates of rm, you are effectively comparing the 2 extinction products in a very roundabout way. As is, this provides no validation/evaluation and, for the purpose of this paper, is meaningless.

25. Line 244: "The Fig. 7 comparisons verify our assertion that errors due to aerosol phase function variation with radius are minor"
This presupposes that your rm estimates are correct, which has yet to be demonstrated. All agreement shown in this figure (or disagreement) is strictly due to the similarity in the 2 extinction products and in no way validates your rm estimate.

Responses: For comments 24 and 25, the whole Section 3.1 is deleted.

26. Lines 265–266: "...uncertainty ranges of OMPS-LP retrievals are calculated from the uncer- tainty in the color ratio extinction coefficients..."
As written, it seems that the authors did not include an uncertainty factor for the distribu- tion width (please clarify). If they did not, then the uncertainty bounds in this figure are misleading and I ask that they include a full accounting of uncertainty.

I also read in the OMPS v2.1 readme "Loss of sensitivity of short wavelengths radiances to aerosols. This effect is caused by Rayleigh and aerosol attenuation of the limb scattered radiation, and becomes most pronounced below ≈17 km and in the southern hemisphere. We advise caution in using LP aerosol extinction data below 17 km and scattering angle greater than 145 degrees for wavelengths 675 nm or shorter. The error bars provided in the daily product data files do not include this term. This error can be reduced by using 745, 869, and 997 nm wavelengths." My concern is with this sentence: "The error bars provided in the daily product data files do not include this term." Given this statement, are CR-error estimates, which are then used to calculate the overall rm error, correct?

Responses: The uncertainty in distribution width is not included in this figure, nor should it be, as the figures are meant to compare which distribution width values best fit the balloon measurements.

Regarding wavelength sensitivity, both figures focus on the Northern Hemisphere and do not discuss altitudes below 16 km.

27. Lines 266–267: "To account for uncertainty in the assumed width we show the results for widths varying from s=1.2 to 1.8"
This demonstrates the sensitivity of your size estimate on the distribution width, which leads to the question of "Which distribution width is correct?" This figure alone provides irrefutable evidence that using a static distribution width is incorrect. This is observed by the disagreement between the 2 profiles (it does not matter what panel you look at) and by plotting the OPC-derived σ profile for this flight. The OPC data show variation in σ that ranged from 1.18 to 2.45

in the stratosphere. It is physically impossible to generate a meaningful rm estimate using a single σ value throughout the profile.

As mentioned above, the authors have neglected the wealth of information on this topic and arbitrarily selected a value of 1.6 and incorrectly cited Wrana et al. 2021 in support of the 1.6 value, but failed to recognize that Wrana et al. showed (e.g., in their Fig. 6) that the distribution width has a broad range.

There is additional information available from the Wyoming OPC record (see my figure above) that can be useful in setting these boundaries. Recent work by our group shows that distribution width is generally between 1.2 and 1.9 (Knepp et al. 2024 https://doi. org/10.5194/amt-17-2025-2024), but this is highly dependent on where and when the measurements are made.

Responses: These figures are used to validate our assumption about distribution width. The distribution widths beyond the displayed range perform even worse compared to the measurements, so there is no need to include them. Additionally, a dynamic distribution width is not the focus of this study.

28. Line 270: "The comparisons in Fig. 9, 10 show the best agreement is for s=1.6. This is consistent with analysis of Wrana et al (2021, 2023)"
Neither of these papers support this conclusion.

Responses: Please refer to the response above for the main comment.

29. Line 271: "...where the particle distributions are unimodal..."

Is this statement made strictly in regard to Fig. 9 & 10 or is this a more general statement. Looking at the Wyoming OPC record I see numerous examples of bimodal distributions well past 25 km. If we limit the analysis to the 2 profiles you showed here then we can fool ourselves into thinking the situation is better than it is (i.e., your analysis indicates that your algorithm performs better when the aerosol distributions are unimodal, but bimodal distributions are ubiquitous). It would greatly benefit the reader to expand this analysis to include more profiles that contain persistent bimodal distributions.

Responses: Thank you for providing this information. The bimodal distribution will be part of our next investigation. Note that Knepp et al. (2024) suggests that is very difficult to retrieve a bimodal distribution using SAGE data.

30. Lines 171–172: "Both the OMPS-LP and balloon data particle radius are ≈0.1 μm at all altitudes..."

This is categorically false and is refuted by the authors' own figures. A cursory look at the figures shows an overall range of 0.05–0.125 μm in Fig. 9 and a range of 0.05–0.3 μm in Fig. 10.

Responses: We mean that most values are around 0.1 μm, but that sentence has been deleted.

31. Lines 272–273: "The particle radius decreases with increasing altitude for both OMPS-LP retrievals and in the balloon data above 20 km."
This is not correct. The OPC rm value increased rapidly above 20 km and held steady until ≈ 21 km where it began to decrease.

Responses: We changed the 20 km to 21 km.

32. Line 278: What is meant by "strongly bimodal"? Quantifying this would greatly aid the reader. Please quantify to remove ambiguity.

Responses: We modified the sentence.

33. Lines 281–282: "The reasonable agreement using s=1.6 shows that this particle width works under both ambient and the aged volcanic plume conditions."
This is categorically false and I fail to see how the authors can make this claim. As is obvious from Fig. 10 this did not work well between 16–20 km where the 2 products differed by up to a factor of 6. Even at 18 km the disagreement remained a factor of 3. This is not good agreement. Please correct this statement.

Responses: There is a bimodal distribution between 16 and 20 km, so it cannot simply be described as a factor of 6. Additionally, among the various distribution widths, s=1.6 shows the closest fit to the measurements.

34. Line 293: "...background -90° to 90° in Fig. 11a, b..." For this plot, what altitudes were plotted?

Responses: Measurement distributions during different observing conditions from the tropopause to 35 km.

35. Line 293: "...background -90° to 90° in Fig. 11a, b..."
It is difficult to understand the classification of this time period at "background" immediately after the La Soufriere eruption and when the atmosphere was still recovering from the ANY event (granted, much of that event already dissipated, but we still see traces in the SAGE data). Please clarify this designation

Responses: It is difficult to find any purely quiescent – there will always be some small uncertainty. Below is from GloSSAC and shows that there are constant aerosol perturbations in the stratosphere. Below is the GloSSAC SAOD with the periods where the analysis takes place. While not perfect, these regions are reasonably representative of background.

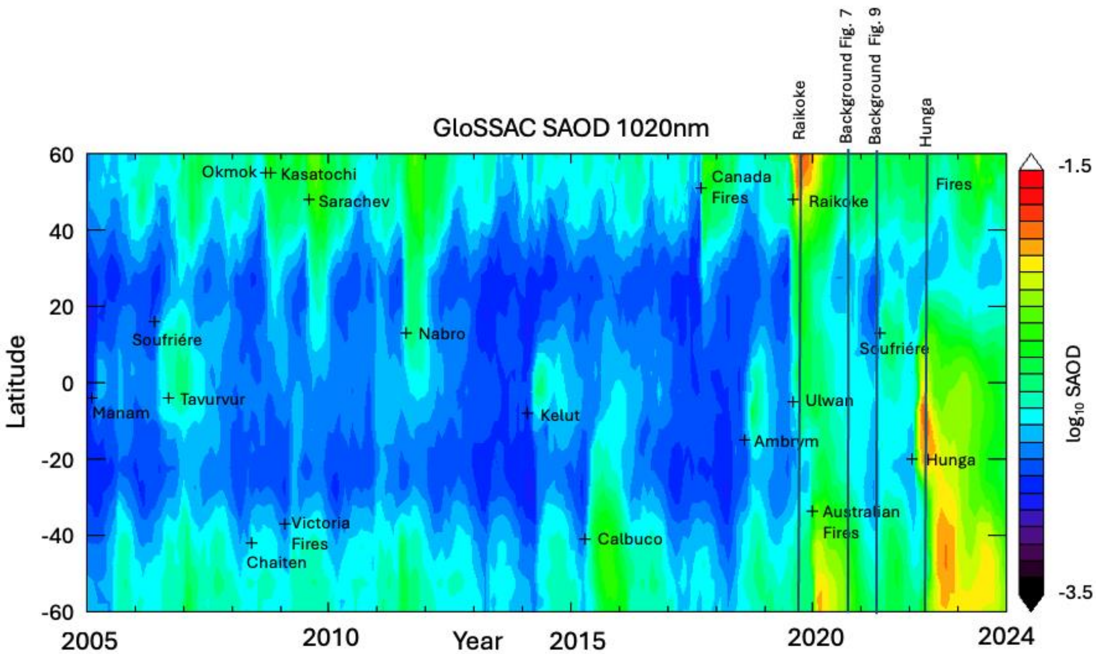

Figure 2. The time series of GloSSAC SAOD with the periods where the analysis takes place.

36. Line 331: While listing uncertainty sources the reader should be reminded of the issues in the OMPS product during elevated aerosol loading (per Bourassa et al. 2007, 2023).

Responses: Bourassa et al. (2023) introduced a tomographic retrieval technique using multiple profiles to remove or reduce the 'arch effect' where the standard OMPS algorithm extends an isolated aerosol anomaly downward at the edges increasing the total aerosol optical depth. This can create an anomalously high SAOD. Bourassa suggested this explains that difference between SAGE and OMPS-LP retrievals. The correction introduced by the Saskatchewan team reduced the SAOD to values consistent with SAGE early in the Hunga eruption period. However, the differences between SAGE and OMPS persist even after the eruption aerosol has dispersed - when the 'edges' have disappeared. The present consensus is that the differences between NASA OMPS and USask OMPS are due to assumptions about the size distribution. This explanation is more in line with our results showing the sensitivity retrieved aerosol properties to the width of the distribution. As a result we feel no need to bring this issue up until it is better resolved.

37. Lines 332–334: "The impact of the distribution width is limited. Especially, Fig. 10c suggest that = 1.6 is a good approximation for the later stage of aerosol evolution."
This is wrong. Even a cursory look at these figures shows the distribution width plays a significant role in the size estimate. Contrary to the authors' claim, Figs. 9 & 10 demonstrate the importance of correctly selecting the distribution width. Further, the OMPS-derived rm in Fig.

10 (c) is 3–6 times larger than the OPC value and the OMPS profile shows a distinctly different structure from the OPC. I am sorry to say, but this difference is not inconsequential:

we cannot declare this a "good approximation". Can the authors please explain to the reader how being off by a factor of 3–6 can be viewed as good?
Please note that this poor behavior is a direct consequence of the static distribution width. What this demonstrates is how a constant σ forces your algorithm toward a particular set of rm solutions. This is a clear demonstration of the inadequacy of this assumption.

Responses: Please referring to the comment 33.

38. Line 347: The reference to Taha et al. 2022 is not appropriate since that paper did not determine the aerosol composition.

Responses: Deleted.

39. Line 364: "The median radius grows through day 30-80. The 0.4 μm peak in the median radius..."

It is not clear to me how this is possible since the authors imposed a CR ratio cutoff (1.1) that, per their description, limited you to particles less than 0.3 μm?

Responses: The limitation due to the color ratio cutoff has been corrected to 0.4 μm in the text

40. Line 398: "We verify our algorithm using SAGE III/ISS..."

As discussed above this exercise in no way evaluated the performance of your rm algorithm. This only tested the agreement between the 2 extinction ratios and, knowing how well the 2 ratios agree. This is a meaningless "verification" since the result could have been guessed before the analysis began (i.e., if the 2 CRs are close then the inferred rm will be close; it proves nothing about the algorithm).

Responses: Deleted.

41. Line 400: "...we validate our retrieved aerosol median radius..."

Claiming your product is "validated" by comparing to 2 OPC profiles (one of which the agreement was quite poor) is misleading to the readers. Please revise to communicate this accurately.

Responses: We responded to this comment above.

42. Line 403: "There are three major sources of uncertainty in our radius calculation..."

Per Bourassa et al. 2007, 2023 and Gorkavyi et al. 2021 the authors should include the bias in their extinction product under elevated aerosol loading as a source of uncertainty/error.

Responses: See response on line 36.

43. Lines 408–409: "However, the good agreement between our retrievals and in situ observations suggests a width of 1.6 is a reasonable value under both ambient conditions and the Raikoke volcanic eruption."
I realize I have hit this point throughout the paper, but since the authors make this claim throughout the paper it seems they are genuinely unaware of how wrong this is. An assumed static distribution width of 1.6 is wrong and should not be done. The authors should know this from their Fig. 10, from Wrana et al. 2021, Wrana et al. 2023, Knepp et al. 2024, and the entire OPC record. This single assumption introduces a massive source of error and directs the algorithm to a predetermined subset of their solution space (this is clearly seen in Fig. 10).

I will note that assuming a value of 1.6 is a reasonable assumption during background periods, though you must still properly account for natural variation, which the authors have yet to do. However, background conditions are not particularly interesting. Further, if we assume $\sigma = 1.6$ as a "valid" assumption for distribution width then why not assume a mode radius of 85 nm? During background conditions the rm=85 nm assumption is just as valid as the $\sigma = 1.6$ assumption. Outside background conditions, both assumptions fail and effectively break the utility of the proposed algorithm.

44. Figure 10: I already spoke at length on Figure 10 and I believe this figure provides incontrovertible evidence that the static σ assumption is wrong. Please consider plotting the OPC σ value as well. Doing so will demonstrate 2 things: 1. a static σ value is incorrect, 2. it will show you why your algorithm fails at reproducing the OPC profile (i.e., the information content of the OMPS data is too low and your estimate is being driven predominantly by the changing σ).

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

Responses: For the discussion regarding static distribution width in comments 43 and 44, please refer to the response above for the main comment.