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 "support" 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.

This is a major problem with this method: using a static distribution width inevitably forces the $\mathrm{r_m}$ 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 $\mathrm{r_m}$ 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 socalled 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 r_m remained steady at ≈50 nm from 16–21 km, while the OMPS method yielded r_m 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".

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 r_m 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 r_m estimates were off by up to -124%, -95%, -28%, 3%, and 21% at the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} 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 r_m.

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

It is for these reasons that I recommend that this paper be rejected for publication.

Brief synopsis of the authors' work

Herein, the authors demonstrate how the size (radius, herein referred to as r_m) 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 r_m. 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 r_m 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.

– 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.

– The authors gloss over glaring issues within the OMPS data product and how these problems will impact their r_m 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 (Bourassa et al., 2007 `oi: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 analysis 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.

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 because it was not done here. There was no validation/evaluation/verification of the algorithm. The authors used OMPS data to estimate r_m and then used a coincident SAGE profile to estimate r_m. 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.

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.

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.

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.

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), r_m, and σ. Please update for clarity.

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

8. line 77: It would be helpful to the reader to define N, r_0, and s here.

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.

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?

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, U_{CR}, 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.

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

16. Lines 160–161: "...common distribution widths (s=1.4–1.6)."
Defining this range of distribution widths as "common" is heretofore unsupported.

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?

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 r_m meaningless (especially under enhanced aerosol loading).

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.

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 analysis and conclusions that are based on this figure. Please recreate this figure using correct uncertainties and accounting for bias under elevated aerosol loads.

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)?

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 comparison, much less one that is based on derived products (products that involve more assumptions 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 r_m, 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 r_m 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 r_m estimate.

26. Lines 265–266: "...uncertainty ranges of OMPS-LP retrievals are calculated from the uncertainty in the color ratio extinction coefficients..."
    As written, it seems that the authors did not include an uncertainty factor for the distribution 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 r_m error, correct?

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 r_m 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.

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.

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.

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.

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 r_m value increased rapidly above 20 km and held steady until ≈ 21 km where it began to decrease.

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

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.

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

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

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).

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 r_m 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 r_m solutions. This is a clear demonstration of the inadequacy of this assumption.

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

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?

40. Line 398: “We verify our algorithm using SAGE III/ISS...”
As discussed above this exercise in no way evaluated the performance of your r_m 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 r_m will be close; it proves nothing about the algorithm).

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.

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.

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 σ =1.6 as a “valid” assumption for distribution width then why not assume a mode radius of 85 nm? During background conditions the r_m=85 nm assumption is just as valid as the σ =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.