Overall

The authors present a comparison of SAGE, OSIRIS, and MAESTRO extinction products (including derived products like stratospheric aerosol optical depth and Ångström exponent) and present a method of correcting MAESTRO extinction to better match SAGE III/ISS. They developed a model as a function of wavelength, altitude, and latitude to transform the MAESTRO data to better agree with SAGE III/ISS.

First, the good! I find this paper to be scientifically interesting, and I see potential utility in this method. If this product can be validated (or at least evaluated to determine the quality and consistency of its performance) then it could be a valuable tool in filling the "SAGE gap". I thank the authors for presenting this interesting study.

Second, the concerns. The paper would benefit from a thorough proofreading for the sake of clarity. It's *not* poorly written, there are just several sentences that do not make sense and several details that are not clear enough to enable the reader to reproduce the methodology. While I have tried to address some of these factors below, I suspect the authors will make several more corrections after a thorough proofreading.

I have 3 major concerns with this paper as written:

1. The authors used v5.2 of the SAGE III/ISS data product while v5.3 is available. This is not a big deal, per se. However, v6.0 is scheduled for release in January 2025 and the changes between v5.3 and v6.0 are aerosol-centric. For example, the so-called seagull effect (i.e., a low bias in the 520, 601, and 676 nm channels) has been nearly completely eradicated. This will undoubtedly impact some of the authors' corrections.

2. The authors tended to focus more on lower altitudes (e.g., 12-18 km, Figs. 8 & 9) where the seagull effect was less prominent. If the authors insist on using v5.2 or v5.3 then it would greatly benefit the reader to see how your correction method performed at altitudes that are more susceptible to the seagull (i.e., 20 - 30 km).

3. The paper would benefit from a more systematic and thorough presentation of how this method improves the agreement between MAESTRO and the other instruments (including more discussion with the agreement with SAGE II). I think this is a missed opportunity to highlight the impact of your work. Something that shows how the overall statistics (e.g., median percent difference, median absolute deviation, etc.) improved (even breaking the samples into "background" and "elevated" aerosol conditions). I don't want to dictate how this is done, these are just suggestions; I am happy to discuss possibilities offline if you prefer.

General Conclusion

This is an interesting paper and I believe it should be published after the authors have had an opportunity to make some modifications. While I listed several concerns with the paper above, and more detailed concerns/corrections below, I do not believe any of these would preclude publication. If I could insist on 1 change to this paper it would be the improvement suggested in the 3rd bullet above. Why? Because this will provide the greatest benefit to the reader for understanding the value of the authors' work. That said, I accept that these changes will be made at the authors' and editor's discretion.

Specific Comments

Line 52 you state that particle size distribution information is available within multi-wavelength occultation measurements. This is true, but this is generally overly simplified in the literature and too often important details are neglected. We addressed many of these challenges, and the impact of various assumptions, within a recently-published paper that you may find interesting and useful (https://amt.copernicus.org/articles/17/2025/2024/).

Line 136: Why not use v5.3? It probably makes minimal difference for the aerosol product, but if you have a reason for 5.2 over 5.3 it would be interesting to hear. As an aside, v6.0 will soon be released (January 2025), which has noticeable changes to the aerosol product (e.g., the "seagull" effect has been largely corrected) and you may wish to re-evaluate your correction factors after that release.

Did you make any corrections to the SAGE III/ISS 520 nm channel? Ray Wang showed an offset in that channel as well as 601 and 676 nm (Fig. 3 in https://doi.org/10.1029/2020JD032430) and we discussed our correction method using a simple power-law correction (Eq. 2 in https://doi.org/10.5194/amt-15-5235-2022). I am not saying these papers must be cited; I provide them for your reference.

Line 159: The 603 and 675 nm channels (520 to a lesser extent) for SAGE III are significantly impacted by ozone and should be used cautiously. Maybe this does not impact your analysis, but it's worth noting. This should all be resolved in v6.0.

Starting on line 159: "During background stratospheric conditions..." I do not understand what the author's are trying to communicate here. Please consider rewording?

Line 173: "...525 nm is excluded, details in Sect. 3.1..." It is unclear why the 525 nm channel is excluded and Sect. 3.1 does not shed light on this either (at least it is not clear to me). This channel looks no worse than any other; why exclude it? Can you please clarify in the paper?

Fig. 3: First off, the authors highlighted the differences at low altitudes (e.g., around 12 km, or so). I am not so concerned about this; did you filter for clouds? If not, then I suspect cloud contamination. Mahesh Kovilakam has a cloud-filter algorithm for SAGE III/ISS (https://amt.copernicus.org/articles/16/2709/2023/) that should be part of the v6.0 release in January (it is currently available in a separate repository cited in his paper) and could be used in your current analysis.

Fig. 3, continued: My main concern with this figure is the similar behavior between the 525/603/779 channels. The SAGE 602 channel was significantly impacted by ozone in v5.2 and v5.3 (biased low by 10-40% depending on latitude/altitude), so I would expect the percent difference in your plot to favor MAESTRO (i.e., MAESTRO should be greater than SAGE...or at least less negative), but it looks like the percent difference becomes *more* negative. Further, the offset changes sign for 1021 nm throughout much of the atmosphere. Overall, this makes it difficult to interpret and I wonder how well behaved the MAESTRO extinction spectra are (sorry, but I know almost nothing about MAESTRO). Can the authors please comment on this? Should we expect the aerosol spectrum to be better behaved?

Fig. 3, final comment: The color scales are difficult to read (this applies to all contour/meshgrid plots in the paper). The authors claimed that the coefficient of correlation was 0.6, but I cannot infer this from the figures. Have you evaluated different color maps and/or limits? As is, I cannot interpret these plots because I cannot distinguish between the various shades. I realize creating these types of figures is quite challenging, so I am sorry to complain about this, but I have a very hard time reading these. Just a suggestion.

Line 257ff: "As aerosol content of the stratosphere varies..." This sentence reads as if composi-

tion plays no role. We demonstrated the impact of composition (smoke and sulfuric acid content) in our paper (https://amt.copernicus.org/articles/17/2025/2024/) and Chris Boone demonstrated variability in the sulfuric acid content (https://doi.org/10.1016/j.jqsrt.2023.108815). Because your analysis involves background, volcanically perturbed, and major wildfire events, this sensitivity should be acknowledged.

Fig. 6 It is unclear how the AE was calculated. I understand this was discussed in Eq. 1 in Section 2.3, but I did not find that section to be helpful in understanding what you did. Could the authors please clarify how the AE was calculated (was it multi-spectral?) and what wavelengths were used?

Line 396: "We note that the bias reported here in MAESTRO measurements is specific to the version 3.13 dataset and would likely be different with updated processing in the forthcoming data versions." This raises several questions:

- When is the next version scheduled for release?
- Do you have any estimate on how much this may change?
- Why not delay publication until you have the next MAESTRO version *and* v6.0 of SAGE III/ISS?

Line 405: "Information about stratospheric aerosol particle sizes in the lower stratosphere can be obtained from the MAESTRO AE values..." This will be challenging because of MAESTRO's limited spectral range. Our group inferred PSD values from SAGE II using 2 channels (525/1020; https://amt.copernicus.org/articles/17/2025/2024/) and it does not appear unreasonable, but the certainty definitely goes down. You may be able to use MAESTRO's 755 nm channel to tease out some more information, but this would require a lot of caution. I am just urging caution with your current statement. However, if you only intend to make general statements (e.g., the particles got "bigger" or "smaller" after an eruption), then what you have is a generally safe statement. Would you please clarify?