The authors would like to thank the referee for taking the time to review this paper and for the many helpful comments that will be used to improve it. The referee's comments/concerns are listed below in red text, while the authors' responses to each comment are written below in black text.

A lot of information and motivations seems to be omitted in the text and it is implied that reader knows that information or assumes from the context. As a result, it makes the paper very hard to comprehend, in particular on the first read. A few examples:
a) In discussion of Fig. 3 and Fig. 4, neither of which are not temporally resolved, authors mention Ambae (p.6, l. 117) and Tonga (p. 8, l. 172) eruptions, which is implied that the reader knows of the magnitude of the aerosol extinction and AE and can easily identify them at the extinction/AE plot. While Tonga eruption was extraordinary, I doubt that even stratospheric aerosol specialists will be able to point that eruption on those plots. On the other hand, Ambae eruption was large, but there were other events which were of comparable magnitude during SAGE III operation time (e.g., 2019 Raikoke eruption), so again, on the 2D extinction/AE histograms these eruptions are extremely hard to identify. As a solution I suggest to either specifically circle those regions on the plots or provide the extinction and AE value ranges for these eruptions.

The specific parts of the distribution attributable to each event is not really all that pertinent to the main message of this paper. In particular, Fig. 2 does not appear all that different essentially if you time resolve it as all of the data tends to follow the distribution shown (depending upon what the stratospheric aerosol load is at that particular time). However, we have taken Figs. 2 and 3 and recreated them for different 4 month periods surrounding events of interest and added it to a supplement.

b) Another example, relates to the knowledge of occultation technique. In the first sentence of Sec. 2 it is mentioned that occultation technique "intrinsically provides vertical profiles of extinction data", which is absolutely true; however, a little more information on the technique should be given. If authors are concerned about the length, then at least some references should be provided.

We have added some minor wording additions and references.

c) I couldn't find explicit information on the the extent of altitudes used for Figs. 2-5 (and as a result for Eq. (2)). Based on the text of the whole paper, I assume it is some range between surface and 17 km as the lowest border and 45 km as the top since clouds and small particles above Junge layer are discussed. Please, provide this information since it is extremely important for interpretation of the results.

The paper mentions that Fig.2 uses "all data in the SAGE III/ISS record prior to 2023", which was meant to imply data at all altitudes, locations, and dates available. We have added a statement to be more explicit.

Maybe this comment will arise from the previous, but I did not quite understand the purpose of the discussion of the regions A-C in Sec. 2. While it is nice to see why people did what they did in the conference talks, here this part makes the paper unnecessarily long and harder to read. As far as I could tell the outcome of almost two page of the description of regions A-C is: the clouds, tropospheric aerosols and data with large uncertainties were filtered out (actually through region D, not through region C). Clouds are a known problem in the stratospheric aerosol retrieval community (cited in the paper Rieger et al. (2019), Kovilakam et al. (2023) or any other paper on stratospheric aerosol retrieval). Similarly, justification for not including tropospheric data could be summarized more briefly. While I might be wrong in my interpretation of the importance of this part, in the current form in my opinion these two pages could be summarized in a couple of sentences without lengthy descriptions. If authors agree with this comment, but would like to keep the descriptions, they could move them into supplement, otherwise context needs to be provided.

Respectfully, the importance of "showing all your work" should be in every paper (given the space when not restricted by the journal) as opposed to conference talks where time is limited and brevity is key. It is true that problems with clouds and/or tropospheric data are sufficiently common that we could have simply stated we excluded these data and been done with it. However, because the final result is so widely applicable, we thought it best to illustrate all of the data and highlight why some data were excluded. Now Regions A and B (and most of C) do not take up much room in the paper despite the theoretical possibility of explaining them away in a single sentence. The discussion of the upper (i.e., larger extinction) portion of Region C as well as Region D is more nuanced and requires detailed explanation and justification as it then creates a potential caveat on the use/interpretation of the results.

While the derived Eq. (2) is highly important, and I anticipate it to be used by many, I think the discussion of its transferability and limitations needs to be included. Few things come in mind:
a) Related to the last bullet in the first major comment is the formula independent of altitude, or is there a specific altitude range where it can be used? Or only low threshold on the extinction coefficient mentioned at p.9 l.195-196 matters? Is there an upper threshold?

In general, the results of Eq. 2 (and Table 1) are widely applicable to all aerosol data in the stratosphere. While the discussion of Region D does leave a bit of a caveat, namely that the very small particles in the upper stratosphere that (likely) result from evaporating aerosol are not completely characterized by Eq. 2, the correction is still in the correct direction so there is no reason not to apply it. It will just be important to understand that some bias from using the "corrected" form of AE interpolation will still be present in this regime. We have added these clarifications to the text.

b) This formula was derived for 756 nm from 520/1021 AE. Is this formula transferable to the other instruments (e.g., SAGE II, SCIAMACHY) which operated at the same wavelengths but under different stratospheric conditions? Can it be used for occultation or it is applicable to limb scattering instruments too?

Given how the data appears to conform to this relationship across the wide range of events in the SAGE III/ISS record, we believe this formula should be applicable regardless of the stratospheric condition. It can certainly be used for occultation, though there is a pseudo caveat for its use with limb scatter. Given the reliability of the occultation technique, it is likely that the occultation data is a reasonable representation of the behavior of this correction as it essentially applies to the shape of the aerosol spectrum. However, it is also likely that the assumptions that go into limb scatter retrievals can/will produce biased aerosol extinction data under certain loading conditions. Depending upon the nature of this bias, applying this correction may yield results that agree "better or worse" when compared with occultation data. However, it is important to remember that the retrieval biases and this empirical correction are independent. Thus, we would recommend using this correction to limb scatter data when applicable and addressing any retrieval biases separately.

c) While the formula was derived for the SAGE III data, would it be the same if calculated with Mie theory using a plausible range of particle sizes (e.g., using the cited in the text Wrana et al. (2021) product)? There is a discussion about the other authors getting much lower uncertainties with Mie theory (p.8 l.168 - p.9 l.185) using SAGE II, but I was wondering why is that so? Is it because the assumption on the unimodal log-normal distribution made by Rieger et al. (2015) and Malinina et al. (2019) is incorrect (possible, and that would be an important statement)? Or is it some sort of SAGE III instrument-specific feature (which is also possible)? Maybe it does not make sense to compare directly with the other authors' results, but it is quite feasible to do this simulation for SAGE III.
While I understand that covering all those point in details might be enough for another paper, at least acknowledging those issues would be crucial for the formula's future application.

The formula is effectively related to the shape of the aerosol spectrum, which is ultimately dependent upon the aerosol properties. Computing it from Mie theory will thus be dependent upon the assumption of those properties, which can be highly varied. One such assumption, as the reviewer suggests, regarding the unimodal log-normal distribution has already recently been shown to be likely in need of refinement (see Boone et al., 2023). In fact, a brief look at the figures of modeled transmission from that work suggest that the unimodal distribution assumption may lead to a reduced AE interpolation bias. As we responded to Reviewer 1, we do not currently have any reason to believe in any biases in the SAGE data that would affect the conclusions of this study at this time. As the reviewer points out, reconciling the discrepancies between theory and observation is definitely a work unto itself and better suited for another paper. Still, we have added some text to the paragraph regarding the comparison between this work and previous studies and welcome the reviewer's feedback.

Throughout the text the term "aerosol(s)" is used as a substitute term for "stratospheric aerosols" and while for certain contexts it is fine (p.4 l.81), in some it leads to misleading statements (e.g., p2. l. 30-32). Please, correct throughout the text.

We have added a statement at the beginning of Section 2, after changing "aerosol" to "stratospheric aerosol" that we will thereafter simply refer to "stratospheric aerosol" as "aerosol" for brevity.

P.1 l.23. "aerosol spectrum" is a jargonism, it is defined at p.2 l.41. Either define here, or change to "extinction as a function of wavelength" or similar.

We have moved the definition to the first instance of this terminology.

P.2 l.30. Please, add "space-borne" before instrument and "stratospheric" before aerosol. Otherwise, you summarize even over such measurements like AERONET (Holben et al., 1998) for the total column.

We have made the previously mentioned change regarding the term "stratospheric" and also included the term "remote sensing" before "instruments". The initial statement of this paragraph is with regard to vertical profiles of aerosol extinction (not total column) being intrinsically measured by the occultation technique.

P.2 l.41. It is not obvious what you mean under "should be slowly varying in almost all stratospheric conditions".

Effectively it means that, unlike trace gas species, there are no noticeable absorption features and the scattering does not change rapidly with wavelength either. At SAGE measurement wavelengths, this is a reasonable assumption.

P.2 l.53. Malinina et al. (2019) cited in this paper a few times showed that the AE on one wavelength pair is not an indicator of particle size. Please rephrase this statement.

That is true, but the AE (or "color ratio") is often referred to in that context. There is a general correlation between AE and particle size as it pertains to volcanic eruptions and SAGE data that is informally used in the literature or in conversation. This statement is not meant to be a definitive one (i.e., one value of AE does not equate to one precise PSD), rather mentioning how AE is often, even if informally, referred to. We have modified the text slightly to: "…often acting as an ad-hoc (albeit imprecise) indicator of effective particle size …"

Fig. 1, please add years of the eruptions and wildfires.

We have added the years of the events as well as various references to them in the text.

P.3 l.61-62 vs Fig. 1. From what I can tell from the Fig. 1, the "dip" is not always an artifact with bias being present just in half of the shown spectra, which indicates that there is some information in there. Can you please elaborate?

The "dip" only refers to a bias in the 520, 602, and 676 nm channels. This was shown much more clearly and explicitly in Wang et al. (2020). It is still a matter of investigation for the SAGE team, though we have reason to believe it is related to ozone spectroscopy. Its actual behavior can vary from event to event, as well as with altitude and aerosol loading conditions. Sometimes it is not as noticeable, particularly in averaged data. It is only slightly noticeable in some of the "spectra" shown in Fig. 1, but rest assured it is there. Note this is different from the bias seen in the 384 nm channel at low altitudes that is the result of a loss of signal in the instrument as a result of very large molecular scattering. We have also made sure to add clarification to what we are referring to in the text.

P.3 l.69. I would list the instruments with the retrieved products at 750 nm.

Added

P.4 l.86. Technically, in Fig. 3 you show log(k_{756_meas}).

Added

Color bars labelling in Figs. 2-5 is needed (I assume it is probability density).

The figure caption states that the "histogram axis shows the fraction of all events used in this figure" so the color bar is a fraction of total events.

P. 8 l. 172: "Tonga aerosol" is a jargonism. Also, please, use consistent naming for the volcano (in Fig. 1 it is "Hunga-Tonga"). Maybe, "aerosols after the 2021 Hunga-Tonga eruption"?

Wording has been changed to be consistent and clearer.

Section 5. Please, update based on the major comments on limitations.

Some wording changes and additional text were added to the conclusion.