

Review of the revised manuscript by Martinsson et al.

We are grateful for your input to our manuscript. We have carefully considered all the comments and made substantial modifications to the manuscript. Some of these are the following:

1. Additional figures in the main manuscript.
2. Discussion on tropospheric particle's impact on the stratospheric aerosol load.
3. Modifications to the discussion on lidar and passive sensors.
4. Modified discussion on how the BD impacts the patterns seen in our figure(s).
5. Further clarification on Hunga-Tonga's aerosol.
6. Changed interpretation of the RF estimate and removed it from abstract and conclusions section.
7. Changed color scales in response to reviewer comment.
8. Added a supplementary figure showing data without extrapolation to high latitudes.

Please find the detailed answers to the comments below.

I thank the authors for their response to the reviewers and for the revised version of the manuscript. The revised manuscript contains some useful additions and clarifications, however I find that several of my main concerns have not been satisfactorily addressed. Some revisions are useful, but they remain rather limited relative to the issues raised in the first review. In its present form, the manuscript still does not provide a sufficiently critical and well-contextualized synthesis for ACP. I therefore cannot recommend publication in the present form and suggest that further substantial revision is needed.

Major remaining comments

1. Structure and logical organization

My previous comment on the structure of the paper has not really been addressed. The authors state in their response that the structure is logical to them, but the revised manuscript still has the same fundamental problem: Sect. 4 continues to mix presentation of additional results, interpretation, and discussion. In particular, Sects. 4.1–4.3 still contain substantial new analysis and quantitative results, while the more critical comparison with other datasets remains concentrated in Sect. 4.4.

I still think that the manuscript would benefit from a clearer separation between results, methodological interpretation, and broader discussion. If the authors prefer to keep the current structure, they should at least make the logic of the section organization more explicit and avoid presenting major new quantitative results in sections labelled as discussion.

We have followed your suggestion and moved three subsections from the discussions to the results section, i.e., sections 4.1 – 4.3 are now 3.3 – 3.5.

2. Spatio-seasonal coverage of nighttime CALIOP data

The authors have now clarified that the missing nighttime CALIOP data at high latitudes were linearly extrapolated to 80° latitude and state that the uncertainty is minor because the affected

region represents a small fraction of the Earth's surface. This clarification is useful, but it is not sufficient.

The issue is not only the contribution of the polar regions to the global surface area. High-latitude stratospheric aerosol perturbations, especially those related to wildfire smoke intrusions, can be spatially localized but optically significant. Therefore, the statement that the uncertainty is minor should be supported by at least a simple sensitivity test, for example by comparing global AOD estimates with and without the extrapolated high-latitude regions, or by showing the maximum possible contribution of the extrapolated domain during major high-latitude events. Alternatively, the figures should explicitly mark where data are extrapolated rather than observed.

As it stands, the extrapolation procedure is described but not quantitatively justified.

We have further clarified the problem by giving the fraction of the surface area affected by extrapolation and by also pointing out, with reference, that high-latitude aerosol events in the summertime could cause increased uncertainties. This was added to Lines 114-119: *"In the global perspective used here the fraction of the earth's surface area affected by the extrapolation is 8.7% at the summer solstice and 3.5% two months from the solstice. In most cases the quantitative impact on the global AOD is small, but in special cases, like the eruption of the Icelandic volcano Grimsvötn on 21 May 2011, the inability to measure high latitudes causes larger quantitative errors (Andersson et al., 2015)."* We have also added Figure S7 showing the data of Figure 3 without extrapolation.

3. Definition and interpretation of background aerosol

The revised manuscript is clearer than the original in stating that the applied method extracts the minimum aerosol loading observed during the CALIOP period and that this operational background may include weak wildfire influence, ATAL contributions, and extratropical tropopause exchange. This partly addresses my previous comment.

However, the manuscript should be more consistent in the way it presents this quantity. It should not be described too strongly as "the" stratospheric background aerosol without reminding the reader that it is an operational minimum derived from the CALIOP period. The CALIOP era was not free from perturbations, and the authors themselves acknowledge that weak wildfire effects and ATAL contributions are included. This is acceptable, but the distinction between an operational minimum, a climatological background, and a physically unperturbed background should remain clear throughout the manuscript, including the abstract and conclusions.

I also suggest avoiding the term "minimum concentration" where the method is based on backscattering. Unless an actual aerosol concentration is derived, "minimum backscattering" or "minimum aerosol loading" would be more appropriate.

The minimum in stratospheric AOD 1998 – 2000 includes weak wildfires as well as other minor extratropical troposphere-stratosphere exchanges. The ATAL was also identified in this period (Vernier et al., 2015). Our evaluation indicates that the background we extracted agrees well with AOD of 1998 – 2000 obtained with SAGE II. With the added material in the first review round, we have given a clear explanation what we mean with the stratospheric background aerosol. This concept is not well defined in the literature, as we point out in the manuscript. We prefer to have a dedicated section on the method to derive "background" aerosol data. This way readers that need to know exactly how we derived the "background" aerosol data can read that in one dedicated place in our manuscript (Section 2.2), where the method is described in detail.

It is difficult to explain in a few words every time the background is mentioned. Designations like “minimum backscattering” does not tell the reader that we are dealing with the background, which we believe would be a draw-back for most readers.

We replaced “concentration”, thank you for pointing this out.

4. Interpretation of the seasonal background signal

The revised discussion of the seasonal LMS variability is improved. The authors now mention the role of Brewer–Dobson circulation seasonality, weakening of the subtropical jet, tropospheric influence on the LMS, ATAL, small wildfires, and seasonal changes in aerosol composition. This is a useful addition.

Nevertheless, the manuscript still does not fully disentangle layer-volume effects from real aerosol variability. Since the seasonal signal in the extratropical LMS is said to coincide with the seasonal variation of LMS volume, the authors should be cautious in interpreting Fig. 4 as evidence of aerosol source or transport variability. I recommend explicitly stating that part of the seasonal cycle may reflect the changing geometry and mass of the considered layers, not only changes in aerosol abundance or sources.

Thank you. A sentence was missing. We have added: “In addition to the size of the LMS, seasonality of the backscattering intensity is connected to a variability in aerosol load.” between the description of LMS size seasonality and the description of annual variability of the aerosol load.

With regards to the reviewer’s comment: “...disentangle layer-volume effects from real aerosol variability...”: We extract and quantify the background and discuss its sources. To sort out all the contributions to the background quantitatively would of course be interesting but that is a different issue that is beyond the scope of this overview.

5. Radiative forcing estimates

This remains one of my main concerns. The authors now state that the Schmidt et al. relationship is based on volcanic sulfate aerosol and is not designed for absorbing wildfire aerosol. However, this is only a minimal caveat and does not resolve the issue.

The manuscript still reports RF values as quantitative results, including in the abstract and conclusions. The approach still does not account for the vertical distribution of extinction, the latitude of the perturbation, aerosol absorption, or the different radiative behavior of wildfire smoke compared with volcanic sulfate aerosol. The statement that volcanic sulfate aerosol is the dominant event type during the period is not sufficient, because some of the most important perturbations discussed in the paper include wildfire smoke, especially the 2019/2020 ANYSO event.

I maintain my previous recommendation: either remove the RF estimates or clearly present them as purely illustrative order-of-magnitude values, with a much more explicit uncertainty discussion. The abstract should not present these numbers as a robust estimate of “climate impact” unless the limitations are clearly stated.

The ANYSO smoke did indeed impact the stratospheric AOD, but over the 17 years studied, volcanic perturbations dominate the variability in the stratospheric AOD. We have replaced RF with AOD in the abstract and removed RF from the conclusions, and we have changed in section 3.5 (previously 4.3) according to the answer to comment l.553-569.

6. Balance of the CALIOP versus solar occultation discussion

The comparison with solar occultation and GloSSAC remains interesting, but the tone is still somewhat one-sided. The manuscript argues that lidar measurements should be given a more prominent role in stratospheric aerosol climatologies, but this conclusion should be balanced by a clearer discussion of CALIOP-specific uncertainties: lidar ratio assumptions, multiple scattering, cloud contamination near the tropopause, sampling limitations, and the fact that the background lidar ratio cannot be estimated with the method used here.

The discussion would be stronger if the authors framed this section less as an argument in favor of CALIOP and more as a balanced assessment of the complementarity and unresolved discrepancies between nadir lidar and limb/occultation techniques.

We agree that the argumentation was leaning towards CALIOP. We have re-formulated that paragraph to clarify that both methods are needed due to their different physical properties and the time periods where data are available. As this manuscript deals with lidar measurements and because the lidar ratio is an important point in CALIOP measurements, we devote the last paragraph to new and future satellite-based lidar systems providing extinction measurements.

Remaining specific comments

- l.284–289: Sect. 3.2 still relies heavily on Figs. S1–S6 in the Supplement. I understand the authors' concern that six full-page figures would interrupt the manuscript flow, but the present section uses these figures for central parts of the scientific interpretation. The authors could consider moving one synthetic version of these figures to the main text or reducing the detailed discussion of features that are only visible in the Supplement.

Thank you for the suggestion that solves the problem. We have inserted two new figures: 4 (from S1 – S3) and 5 (S4 – S6). We have kept the full-size versions in the supplement.

- l.302–304: The clarification of “overshooting plumes” is only partly satisfactory. The phrase “overshooting plumes reaching above the main effluents” remains awkward and should be rephrased more clearly, for example as “plumes injected above the main aerosol layer”.

We changed the text according to the reviewer's suggestion.

- Fig. 3 / l.311–315: The caption is clearer than before, but the plotted quantity remains unusual: “Global AOD contribution per degree of latitude.” Since this is not the standard way SAOD is presented, the normalization should be explained more explicitly, either in the caption or in the Methods. The figure should also clearly distinguish missing/extrapolated values from physically low values.

We have added further explanation of the color scale. We replaced the color for low values in Figure 3 and Figure 8c (old Figure 6c) where we also extended the scale to lower values.

- l.320–323: The sentence on the Australian wildfires remains confusing: “rapidly (half-life 10 days) lost 90% of the aerosol...” A half-life of 10 days does not mean that 90% is lost in 10 days. Please rephrase and clarify whether the 90% loss refers to a specific period, altitude range, or aerosol component.

Thank you, we have re-phrased to make the statement clearer.

- L.330–331: “The LMS, the last stop passage...” is still awkward and colloquial. Please rephrase more formally.

We have rephrased and hope that the new formulation works better.

- L.293–296 and L.538–540: The discussion of the Hunga aerosol remains underdeveloped. The manuscript repeatedly links the event to “intense volcanism–sea interaction,” but it does not explain how this inferred composition affects either the lidar-ratio behaviour shown in Fig. 2l or the resulting AOD interpretation. If the authors are not prepared to discuss this link, they should avoid implying one.

Maybe we misunderstand this comment: Our methodology does not include theoretical modelling of lidar ratios. From our fitting method applied to the backscattering altitude distribution, we obtain the effective lidar ratio and a correction for attenuation in an iterative computation without making assumptions on the particle chemical composition, as presented in Martinsson et al. (2022). “Intense volcanism – sea interaction” is explained by Seabrook et al. (2023) and Mastin et al. (2024) (referred to in this manuscript) and several other papers referred to in Martinsson et al. (2025) but not in the present manuscript that overviews a 17-year period. In Martinsson et al. (2025), dealing with the Hunga eruption, we discuss and interpret how this interaction with the sea affected the volcanic effluents.

At L.293-296 we have added “..., whereas aerosol formation from SO₂ could explain only ~30% of the AOD...” with reference to Martinsson et al. (2025).

- L.460–467: The statement that the transport pathway is supported by Fig. 6 should be softened. Fig. 6 may be consistent with the expected Brewer–Dobson transport pathways, but it does not by itself demonstrate them. The same applies to the claim that CALIOP can constrain modelling efforts “by reconstructing the CALIOP observations in Fig. 6”; this should be made more specific. What model quantities would be constrained: source altitude, vertical aerosol distribution, transport pathways, or aerosol formation rates?

Regarding the comment “...does not by itself demonstrate them.”: We discuss BD transport, aerosol formation pathways and stratospheric altitude-latitude structure of air age and arrive at our interpretation of the mixing ratio structure of the stratospheric aerosol before L.460-467. L.460-461 says that the tropical dBD air eventually moves polewards, where in the extratropics it follows the stratospheric circulation downwards. The tropical sBD and dBD air do not cross each other when transported pole- and downwards, although there could be some mixing. This pattern can also be observed in stratospheric air age dependence on latitude and altitude. L.462-465 is an overview on the OCS importance for the stratospheric background aerosol, and the remaining 2.5 lines are addressed next.

Regarding the comment “...model quantities...”: We have added “...to quantify sulfurous aerosol sources...” to clarify what we mean. It now reads: “*High-resolution lidar data, like that of CALIOP, can be used to constrain modeling efforts to quantify sulfurous aerosol sources by reconstructing the CALIOP observations in Figure 8 to understand the sources of the background aerosol.*” The low aerosol load in the tropical sBD contains mainly sulfurous aerosol

from SO₂ and tropospheric particles, whereas OCS remains in its original form during transport polewards in the sBD. In the tropical dBD aerosol formed from OCS is added.

- l.481–485: The explanation of enhanced lower-stratospheric aerosol load by compression during downwelling remains too categorical. Tropospheric sources and exchange processes are now mentioned elsewhere, but the statement here should be phrased more cautiously and acknowledge that these processes may also contribute to the LMS aerosol load.

We have added: “Mixing across the extratropical tropopause culminating in the late summer with the ATAL affects the aerosol load in the LMS but does not affect the mixing ratios appreciably (Figure 8a). However, somewhat higher scattering ratios are found in the ATAL region (15 – 45° N and 13 – 18 km altitude (Vernier et al., 2015)) compared with the same region of the southern hemisphere. In monthly resolution, rather than the yearly resolution of Figure 8, enhanced aerosol load in the ATAL area is clearly visible in July – September 2013 (Martinsson et al., 2017).”

- l.503–529: The practical significance of the lidar-ratio correction remains unclear. The manuscript states that AOD corrections were applied when the effective lidar ratio deviated by more than 5%, but then concludes that the overall change in AOD due to the correction was minor. This should be explicitly discussed in relation to the stated motivation of making CALIOP extinction quantitative.

The SAGE III/ISS – CALIOP comparisons by Kar et al. (2019) and Kovilakam et al. (2023) illustrate the importance of this part of our investigation as their results indicated more than 50% discrepancy, which they accounted to unknown lidar ratio. The lidar ratio is very important. Our results show that assuming 50 sr will work satisfactory for most aerosol events. We have added a paragraph that sums up our findings on the effective lidar ratio: “Altogether the changes in AOD from the corrections due to deviant lidar ratio were found to be minor, the largest correction (-12%) was applied to the AOD of the Raikoke eruption. However, we could not statistically quantify all major aerosol events, most notably the Nabro eruption in 2011 (Figure 2). Our results show that assumption of an effective lidar ratio of 50 sr works satisfactory in most cases in the 17-year period studied when the stratospheric aerosol is influenced by volcanism or wildfires. The applied method to obtain the effective lidar ratio cannot be used for optically thin layers like the background aerosol.”

- l.553–569: The RF estimate still needs stronger caveating. The manuscript now states that the Schmidt et al. relationship is not designed for absorbing wildfire aerosol, but the RF values are still presented as quantitative results. These estimates should either be clearly described as illustrative/order-of-magnitude values or accompanied by a more complete uncertainty discussion.

We agree that we need to do more here. We do not wish to call it “order of magnitude” or “illustrative” but instead added the word “simplified”. We have added “This simplified estimate of...”, which we believe is a well-balanced statement.

- l.661–665: The discussion of Brewer–Dobson transport in the Conclusions should be phrased

carefully. Some of this material reflects established stratospheric transport knowledge rather than a conclusion newly demonstrated by this study. The authors should distinguish more clearly between results obtained here and background interpretation.

Our aim is of course to explain our results. To explain Figure 8a (previously 6a) we must refer to the BD (and aerosol formation paths and the altitude-latitude age structure of stratospheric air). We have reformulated to first mention our observations and then give our interpretations with respect to BD, aerosol formation and stratospheric age structure. This way, there is a clear separation of our results and our interpretations of them.