Below follows our answers in blue text to comments and questions from the three reviewers. We thank the reviewers for their efforts which has helped us to improve our manuscript.

Reviewer 1

Thank you for your useful comments.

Review of "A global view of the stratospheric background, volcanic and wildfire aerosol in the CALIOP era (2006 – 2023)" by Martinsson et al.

This paper deals with the stratospheric aerosol loading, both the background and perturbations from the volcanoes and wildfire events observed during the mission lifetime of spaceborne lidar CALIOP on-board the CALIPSO satellite. It attempts to delineate the background aerosol in nine different parts of the stratosphere on the assumption that the stratospheric background levels can exist at different levels and time. After subtracting this background, the authors discuss the strong perturbations from several volcanic events and wildfire events, for which they derive the lidar ratio and obtain the AOD. They highlight the importance of space lidars like CALIOP to characterize the stratospheric aerosol burden, in relation to the solar occultation and limb scatter instruments. The paper is within the scope of ACP and generally well written. However, in several places the information is generally well known, in particular the overall impacts of the volcanic and wildfire events in recent years. I am a little unsure of the motivations for this work, particularly because a new level 3 CALIOP stratospheric aerosol product with several updates is currently available--perhaps the authors could better emphasize the new information from this study. In any case, the paper presents an independent assessment of CALIOP stratospheric measurements and I recommend publication of the manuscript. I have a few comments to improve the paper in no particular order.

"...new information from this study...": Here we are the first to present an overview of the entire era of CALIOP measurements. We correct attenuated data, which is beyond standard praxis for official CALIOP data products. We undertake effective lidar ratio estimates for many stratospheric aerosol events, discuss and undertake AOD corrections when the lidar ratio deviates from 50 sr to make extinctions obtained by CALIOP quantitative. We present new methodology to estimate the stratospheric background aerosol and find that the AOD of that background agrees well with solar occultation measurements taken in the volcanically quiescent period 1998 – 2000 and that the extracted high-resolution background well illustrates stratospheric aerosol formation, sources of the background aerosol, stratospheric transport paths and stratospheric age of the air. In order to further put our results into context we also discuss discrepancies between lidar and solar occultation measurements presented elsewhere. We feel that this manuscript contributes strongly to this research field.

1. The authors have used version 4.51 level 1 CALIOP backscatter measurements for their study. Why not use the latest version 5 data product, although the results presented here should not change much. Also, the version 2.0 level 3 stratospheric aerosol product as available in the CALIPSO database incorporates the schemes to address the low energy laser shots which have been impacting the measurements after 2017 during which time several of the strong volcanic and wildfire events discussed in this paper occurred. It is not clear to me if the authors addressed the low energy shots in some way. I wonder how their background component compares with the official product.

We have been working with CALIOP level 1B data since 2012 and published our first paper in 2015 (Andersson et al.) where we, among other things, extended the work of Vernier et al. (2009, 2011) to include the LMS. We have since then developed the code operating at the CALIOP data, most recently (2022) included a correction for attenuation which also includes an estimation of the effective lidar ratio. The version 5 was not available when we processed the Level 1 data. Low energy laser shots were excluded.

The CALIOP level 3 product for the stratosphere (Kar et al., 2019) is based on level 1B with an intermediate step via level 2. They undertake filtering of the data with respect to clouds around the tropopause, PSCs and SSA, similarly to our processing (Friberg et al., 2018; Martinsson et al., 2022). We treat the lidar ratio differently to that of the CALIOP stratospheric level 3 product, where they set a constant lidar ratio of 50 sr. Instead of using a constant lidar ratio, we estimate the effective lidar ratio (effective because CALIOP is affected by multiple scattering) in a computation that also corrects for attenuation. We run our main evaluation that corrects for attenuation with the lidar ratio 50 sr and correct afterwards for aerosols with effective lidar ratio deviating from 50 sr. Based on a separate investigation (i.e., not in the main evaluation) of dense aerosol layers we find that sulfate-dominated aerosol from different volcanic eruptions have different effective lidar ratio (manuscript, Figure 7a) and that the different wildfire aerosols also differ in effective lidar ratio. We acknowledge the value of CALIOP level 3 stratospheric product. However, we find that the methodology we use supply effective lidar ratios suitable for volcanic eruptions or wildfires individually. We also correct for attenuation which is critical for identification and quantification of aerosol processing the first months after a wildfire or volcanic eruption. We add a sub-section in section 2.1 to explain the relation between our data set and the CALIOP level 3 product.

2. line 34--can the seasonally present and anthropogenically sourced ATAL be considered as background aerosol?

In this place we added a short clarification. In the methods section we clarify that we extract the minimum aerosol loads during the 17-year period studied. This background is caused by tropospheric air constituents entering in the tropics in the BD circulation, the ATAL and exchange across the extratropical tropopause affecting the LMS that is not readily detected as individual aerosol events.

3. line 41--suggest adding more recent references from the works of Fromm and co-authors.

We have added Peterson et al. 2025 (https://doi.org/10.1038/s41612-025-01188-5).

4. line 66--suggest adding SAGE II/III references. Also please spell out the acronym GloSSAC and OSIRIS in line 72.

We have spelled out the acronym. The Sato reference cover the early years of solar occultation measurements. We refer to several papers dealing with SAGE data just a few lines below, and still other papers on SAGE on other places in the manuscript.

5. line 101-- Add "respectively" after " ...300m"

We have changed this according to the reviewer's suggestion.

6. Line 104.--Add MERRA 2 reference

We have added: Gelaro, R. et al. The modern-era retrospective analysis for research and applications, version 2 (MERRA-2). J. Clim. 30, 5419–5454 (2017).

7. The methods section (2.1) needs to be expanded a little bit. For instance, in line 109 the authors mention the depolarization of the signal, without first mentioning the measurements in the perpendicular channel. Please add a few sentences on the perpendicular channel at 532 nm and 1064 channel in CALIOP measurements. What was the threshold used for cloud depolarization? Since the authors do not attempt any validation, it is not clear as to how much clouds might impact their lowermost stratosphere results.

Thank you, we have added the information.

8. Line 158 and later--what is meant by "beside"--is it the top of the layer?

We have changed this to "horizontally beside"

9. Figure 5--May be point out that the vertical scales for the different regions are different--it's a little difficult to read all 9 panels in this plot.

We have made the vertical scales the same in all the 9 figure parts, implying that stratospheric parts with low average backscattering become narrow. We have not made any changes in the figure.

10. Line 380 "the order 100 days"--please rephrase

We have changed this to approximately 100 days.

11. I think the authors should discuss the fidelity of their lidar ratio estimates presented, which impact the AOD calculations crucially. I didn't find any comparison with estimates by other authors e.g. Prata et al. (2017) gave 69 sr for PuyeHue Cordon Caulle, much more than ~55 sr shown in Figure 7a. The lidar ratios for the Australian fires of January 2020 were ~ 100 sr as retrieved by a Raman lidar in Punta Arenas (Ohneiser et al., 2020, https://doi.org/10.5194/acp-20-8003-2020), again much higher than estimated here (need to define Fi20 in Figure 2). Any clues as to why the Raikoke lidar ratio is distinctly lower (~45 sr) than others? In particular the latter might be relevant to the recent debate about presence of smoke in Raikoke plumes (Ohneiser et al., 2021, https://doi.org/10.5194/acp-21-15783-2021 etc.). Are some of these differences coming from the multiple scattering factor? Figure 7a shows the Effective Lidar Ratios whereas Figure 7d shows Lidar Ratios--may be I am missing something here.

We estimate effective lidar ratios which are applicable to measurements (like CALIOP) that are affected by multiple scattering. Prata et al (2017) estimated the lidar ratio applicable on measurements that are not affected by multiple scattering. The results by Ohneiser et al. (2020) on 2020-01-09 around 04:00 (longitude -70.9, latitude -53.2) was S = 76 sr. The closest CALIOP measurement in space and time that we evaluated was taken on the same day at 04:05, position (-43.4, -53.1) with S = 75 sr. The day before (-57.2, -50.0) S = 70 sr and the day after (-55.0, -57.1) S = 71 sr. All these three measurements belong to the fires taking place in the last days of 2019, category B (outside the vortex) and are the three highest effective lidar ratios obtained in this category.

We have added comparisons with Pata et al. (2017) and Ohneiser et al. (2020) in the manuscript.

12. Line 479: "unverified assumption of a lidar ratio of 50 sr"-- in a recent paper in JGR, Deshler and Kalnajs, 2026, https://doi.org/10.1029/2025JD045262, from decades long OPC measurements provide a single value for the stratospheric aerosol lidar ratio of 49.9 sr at 532 nm.

Here we mean that we could not verify by the method we use. We change “unverified” to “commonly used”.

13. Add unit of extinction coefficient to the color bars in Figure S4-S6.

Thankyou, we have added the information in the figure captions.

Reviewer 2

Review of manuscript by Martinsson et al., “A global view of the stratospheric background, volcanic and 2 wildfire aerosol in the CALIOP era (2006 – 2023)”.

The manuscript by Bengt Martinsson and coauthors presents observations of stratospheric aerosol by CALIOP instrument over 17 years. The study identifies stratospheric aerosol perturbations generated by volcanic eruptions and wildfire outbreaks, analyses the seasonal variation of background aerosol abundance and discusses the validity of CALIOP-derived stratospheric AOD in consideration of the estimated lidar ratio of aerosols produced by various perturbation events.

The study is built on the methodology previously presented by Friberg et al. (2018) and Martinsson et al. (2022), uses the same approach and targets the same scientific questions, while presenting the updated post-2019 CALIOP observation record. The new aspects of this study – with respect to the previous publications on the topic by this team – are the consideration of variable lidar ratios and the analysis of background stratospheric aerosol variability. In addition, the paper discusses the validity of CALIOP-derived AOD by referring to the solar occultation data from the literature and provides an estimate of radiative forcing using a simple relation between AOD and RF. The conclusion of the study points out the advantages of satellite lidar technique and the need to sort out the discrepancies between active and passive aerosol remote sensing.

While the stratospheric aerosol variability is a topical research subject, whereas CALIOP observation record is obviously of great value for stratospheric aerosol science, I find that the scientific novelty of the manuscript remains limited. At the same time, the scientific value of some of the new results is reduced by several shortcomings discussed below. In general, the manuscript currently reads as an incremental extension of earlier work by this group of authors rather than a sufficiently mature synthesis for ACP. I therefore recommend substantial revision before the manuscript can be reconsidered.

Despite these dismissive words we find the general and specific remarks by the reviewer constructive and useful.

“…scientific novelty…: See answer to the introduction of reviewer 1, starting with: “…new information from this study…”

Our results indicate that the global stratospheric AOD at background conditions based on CALIOP agrees with data from SAGE II at altitudes above 15 km (we compare with Solomon et al. (2011) who used that limit). Despite that, strong disagreement (more than 50%) has been reported below 17 km altitude during background conditions by the team responsible for the important GloSSAC dataset. We point out that the results we present are obtained with methodology that disagree with the more commonly used limb-viewing techniques, in particular solar occultation. We cannot postulate that one method is better than the other. The next step would be to conduct new studies to find further explanations and, if needed, a more representative long-term record of the optical properties of stratospheric aerosol. We feel that our manuscript is a contribution to a sound and vivid scientific discussion.

Major/general remarks.

1. Structure and logical organization of the manuscript

The repartition of the presented material between Results and Discussion is disproportional. In practice, the Discussion section (Sect. 4) is not much different from the Results section, as Sects. 4.1–4.3 continue to describe and extend the results shown in Figs. 4–7, whereas the actual critical discussion is largely restricted to Sect. 4.4, where the CALIOP-derived AOD is compared with solar occultation-based records and GloSSAC. The authors should reconsider the organization of the paper and the naming of sections in order to improve the logical flow and better distinguish presentation of results from their discussion.

The basic results are presented in section 3. In section 4 we discuss various aspects of the results. Usually, you need to present your findings in a particular order. It is sometimes difficult to draw the line between results and discussion in the sequence. Elements of discussion are present in section 4.1 and onwards. The structure of the manuscript is a choice that is logical to us.

2. Discussion and literature review.

While the Discussion section takes up the most of the manuscript, it finally lacks a critical review of the results presented in the context of available literature on the topic of stratospheric aerosols. I got an impression that the authors strongly prefer auto-citations whilst largely ignoring the relevant literature.

Related to this, while the Discussion section takes up a substantial part of the manuscript, it still lacks a sufficiently critical review of the results in the context of the broader literature on stratospheric aerosols. The discussion relies heavily on the authors' earlier work and could engage more directly with relevant literature, especially in relation to the definition of background aerosol (Sect. 4.1), the interpretation of the CALIOP–solar occultation discrepancies (Sect. 4.4), and the implications for climatological datasets such as GloSSAC. As it stands, the manuscript gives too little space to alternative interpretations and external context

"auto-citation": We find this comment somewhat unfair. The first author has authored or co-authored 18% of the references. If we exclude references to unique long-term aerosol chemical information from our papers from the aircraft-based project CARIBIC, the number becomes 10% (including methodological references). See also below on GloSSAC.

"the definition of background aerosol": We refer to 2 papers on the concept "background aerosol", 6 refs on the origin of the background, 1 ref on transport, 3 refs on stratospheric age to undertake this discussion on the stratospheric background aerosol. (None of these references is auto-citations.) See also the answers under point 4.

“the interpretation of the CALIOP–solar occultation discrepancies”: As pointed out by Thomason et al (2018) mixing data from many sources is complex. As far as we know, the reason behind the discrepancies is still an open question. We discuss GloSSAC based on 3 refs from their team and we conclude with “optimally combine solar occultation and lidar measurements for future long-term records”. Given that we have estimated lidar ratios we also think that it is necessary to discuss the more than 50% discrepancies reported elsewhere, as the lack of lidar ratios were given as the main explanation for the discrepancies in that study. Our opinion is that our discussion in section 4.4 is open. We want the discrepancies resolved, nothing else. See also the answer to the introduction made by this reviewer.

3. Spatio-seasonal coverage of CALIOP nighttime data.

Another important issue concerns the spatio-seasonal coverage of the nighttime CALIOP data. According to Sect. 2.1, only nighttime CALIOP measurements are used in the general evaluation, following Friberg et al. (2018) and Martinsson et al. (2022). However, nighttime CALIOP observations do not provide continuous coverage in the polar day regions around the Summer solstice, the nighttime coverage is limited to about 60° latitude (see Fig. 4 in Friberg et al., 2018). In this light, the latitude–time presentation in Fig. 3 requires clarification, because it appears to show continuous coverage up to +/- 82° regardless of season. The authors should explain how the missing data in the polar-day regions were treated, whether any interpolation or averaging procedure was applied, and what implications this has for the discussion of aerosol variability at high latitudes, which are frequently affected by wildfire smoke intrusions.

Thank you for pointing that out. We forgot to explain in the methods section that we extrapolated the data. In the new version we have included such a description in the methods section.

4. Factors of background aerosol variability.

I also have concerns regarding the treatment of background aerosol variability. First, although Sect. 2.2 and Sect. 4.1 provide an operational procedure for extracting the background aerosol, the physical definition of “background” remains insufficiently clear. In Sect. 4.1, the authors themselves note that stratospheric background aerosol is “not a well-defined concept,” and contrast their approach with the definitions used by Solomon et al. (2011) and Kremser et al. (2016). In the present manuscript, the background level is established from the cleanest 8-day periods in nine latitude–altitude regions (Sect. 2.2; Fig. 4), but it is not fully clear to what extent this estimate can be interpreted as a true background state rather than as the minimum aerosol loading observed within an already perturbed CALIOP era. This is particularly important now that the stratospheric influence of wildfires has become more frequent. The authors should clarify whether recurring wildfire-related injections, including weaker or secondary summertime smoke emissions, are conceptually part of the background or excluded from it.

A good point. The method does indeed catch the minimum. This means that weak influences from wildfires and contributions from ATAL are included in the method used. Then we come back to the question: what is the background? We see the background as the aerosol load when no clearly detectable aerosol events from volcanism or wildfires (or possibly other phenomena) are affecting the stratosphere. The background based on the used methodology thus extract the minimum, which, making reasonable assumption on the lidar ratio, agrees well in terms of stratospheric AOD with the background according to SAGE II in the volcanically quiescent period 1998 – 2000. We have clarified in the revised manuscript.

Second, I am not convinced that the seasonal variation of the background aerosol burden shown in Fig. 4 can be interpreted straightforwardly in terms of aerosol transport and source processes. In Sect. 4.1, the authors themselves note that the seasonal cycle in the extratropical LMS closely follows the seasonal variation of LMS air volume, citing Appenzeller et al. (1996). This suggests that a substantial part of the observed variability may simply reflect geometrical or layer-volume effects, rather than a true seasonal modulation of aerosol abundance. I therefore encourage the authors to discuss this point much more carefully and to distinguish more clearly between changes in aerosol amount and changes induced by the seasonal structure of the layers themselves. In addition, the interpretation should better account for processes that are likely to affect background aerosol seasonality, such as uplift of pollution aerosol in the Asian monsoon anticyclone / ATAL region (Vernier et al., 2015), tropical upwelling, and the convective transport of relatively clean air across the tropical tropopause. Without a more careful discussion of these competing factors, the physical interpretation of Fig. 4, and more generally its relevance for understanding stratospheric aerosol processes, remains unconvincing.

You are right. We focused too much on giving a straight presentation, without branching: Poleward transport in the BD circulation maximizes in the winter resulting in increased extratropical downward motion of the stratospheric aerosol layer in the spring resulting in low aerosol load in the summer LMS when the mass transport across its upper boundary is at minimum. The latter also coincides with a weakening of subtropical Jetstream which increases the tropospheric influence on the LMS. As the transport from above starts to increase late summer/early fall we also have influence from ATAL (Vernier et al., 2015) and wildfires too small to detect individually. The chemical composition of the LMS aerosol in that period differ from winter/spring/early summer by having a larger carbon than sulfur content (Martinsson et al., 2019). The change in composition can be caused by the ATAL and/or small wildfires which thus contribute to the effect of the large-scale stratospheric circulation in the build-up of the NH LMS aerosol load during late summer and fall. We have expanded the discussion in the manuscript accordingly.

5. Radiative forcing estimates

Finally, I find the radiative forcing estimates to be of rather limited scientific significance in their current form. The manuscript converts stratospheric AOD into effective radiative forcing using the simple relationship derived by Schmidt et al. (2018) for volcanic aerosol. However, this approach does not account for the vertical distribution of extinction, the latitude of the aerosol perturbation, or differences in aerosol absorptive properties. These limitations are particularly important when wildfire aerosol is considered, because the relation derived by Schmidt et al. for volcanic aerosol cannot be assumed to apply directly to carbonaceous smoke aerosol. Given the very large uncertainty associated with such a simplified estimate, and the lack of a sufficiently critical discussion of these limitations, I believe that the RF values (which are among the main conclusions in this study) should either be accompanied by a much more comprehensive uncertainty discussion or removed from the paper.

The work by Schmidt et al. (2018) provides valuable means to explain the magnitude of stratospheric perturbations to those not familiar with aerosols. We have clarified in section 4.3, especially that the conversion is not directly applicable for smoke aerosol. In the conclusion we clarify that we use a simplified relation to obtain effective radiative forcing from AOD.

Specific remarks

Fig. 2 would benefit from explicit event labels within each panel rather than relying solely on the caption. It would also be useful to indicate, where available, lidar ratio estimates from the literature for the same events to facilitate comparison with previous studies and discuss the differences.

We agree that labels would be useful and add them. We estimate effective lidar ratios that differ from commonly reported lidar ratios due to influence from multiple scattering. Effective lidar ratios are preferable when the influence from multiple scattering cannot be estimated (Martinsson et al., 2022). We have clarified in the manuscript. See also the answer to reviewer 1, comment 11.

Sect. 3.2: An important part of this section (p.10) relies on Figs. S1–S6 in the Supplement. The authors should reconsider whether some of this material is essential to the scientific argument and should therefore be moved to the main manuscript; otherwise, the discussion in Sect. 3.2 should rely less heavily on supplementary figures.

In a way you are right. We had some thoughts on that before placing them in the supplement. If inserted in the running text, the six detailed full-page figures will substantially disrupt the reading of the text.

l.252. Please clarify what is meant by “overshooting plumes” in the context of the Raikoke eruption.

Plumes that reach higher up than the main effluents. We have clarified this.

Fig. 3. What parameter is reported here: integrated backscatter coefficient or SAOD?

From the figure caption: “Color scale: Global AOD contribution per degree of latitude, i.e. the sum over latitude is the total AOD at any given time.”

Fig. 3. Please ensure that fill values are visually distinguishable from physically low values. In addition, as noted above, the treatment of missing nighttime CALIOP coverage in polar-day regions should be explained.

Yes, see answer above.

l.269. “rapidly faded”: as can be inferred from Fig. 3, the ANYSO perturbation persisted for more than one year.

We have clarified that most of the AOD (~90%) from the wildfires was lost with a half-life of 10 days.

l.277. “the last stop”: too colloquial, consider rewording

Thank you, we have rephrased.

l.289. The relation between the volcanism-sea interaction for Hunga eruption and lidar ratios reported in Fig. 2l should be explained, in particular how the inferred particle composition associated with volcanism–sea interaction is expected to affect the lidar ratio.

The lidar ratio depends on the particle size distribution, shape and composition. We are not prepared to make any such predictions.

l.372-373. I do not follow how the transport pathway in the dBD branch can be inferred directly from Fig. 6a. Please clarify the argument or refer to the specific feature of the figure that supports this interpretation.

We describe the transport pathways, stratospheric air age and chemical processing and infer Fig. 6a (and b) based on these phenomena, not the other way around.

l.392-393. Please clarify how exactly the vertical resolution of CALIOP data can be used to constrain modeling efforts.

We have added such an explanation.

l.406-407. I am not convinced by the interpretation of higher aerosol abundance in the LMS by “compressing” process. Could it rather be due to tropospheric sources?

As can be seen in Fig. 6a the scattering ratios in the LMS is similar to those higher up, implying that impact from other aerosol sources is small (except the small features in NH LMS mentioned in the text). Air moving downwards becomes compressed due to the altitude-dependence of atmospheric pressure, hence the high absolute aerosol load in the LMS.

Fig. 8. The thin grey line in Fig. 8a is barely visible and appears to differ only marginally from the corrected AOD time series. Since Sect. 4.2 concludes that the overall impact of the lidar-ratio correction is minor, the authors may wish to comment more explicitly on the practical significance of this correction for the long-term CALIOP record.

One motivation for this study was to make CALIOP measurements quantitative with respect to extinction. To do that we need the lidar ratio. Correction of results obtained with fixed lidar ratio (i.e., 50 sr) depends on the actual lidar ratio and an additional effect from the level of the AOD. For the volcanic eruptions and wildfires studied here we show that the effect connected to the level of the AOD was minor.

l.538-539. Which version of GloSSAC is referred to here?

2.2, we have added that in the text.

1. 571-574. I am not sure that the basic description of stratospheric transport by the BDC should be presented as a conclusion of the present study.

Thank you for spotting this mistake. We have changed the wordings to clearer connect with our observations.

Reviewer 3

We thank the reviewer for the useful comments.

General comments:

This paper provides an overview of CALIOP observations of aerosols in the stratosphere covering the entire CALIPSO mission period from 2006 to 2023. The paper is in principle of interest to the stratospheric community and should eventually be published in my opinion. There are, however, many little inconsistencies or unclear statements that should be corrected (see specific comments below). Two of the comments are more general and are mentioned here:

1. The terminology used throughout the paper is often not very precise: “aerosol backscatter” is often used. In my opinion this is a very vague term and the correct terms probably are “backscattering coefficient” or “volume backscattering coefficient”? Are the units correct, i.e. only “1/sr2?. Perhaps different terms are used in the CALIOP community. It would in any case be good to briefly define the quantity at the beginning. Perhaps I’m wrong, then I’m happy to be informed.
2. The rescaling of the presented AOD values to the contribution of each degree latitude is misleading in my opinion. Each atmospheric column (or latitude band) certainly has an AOD value that can be calculated by vertically integrating the extinction coefficient profile. The global mean AOD should be determined by averaging these values (simply put). Your way to depict the results makes it unnecessarily more complicated to compare with other studies. Please consider change the way the results are depicted.

1. When we discuss backscattering quantitatively it is the average backscattering of a stratospheric part, which when multiplied with the lidar ratio becomes the AOD. We present backscattering results instead od AOD of the stratospheric background aerosol because we cannot use our method to estimate the lidar ratio in thin aerosol because of too high dS/dR. We have clarified this in the methods section, in section 4.1 and in the captions of Figure 4 and 5 and use the designation “average backscattering”.

2. The weighting corrects for variable surface area of latitude bands. If latitude-weighting is not undertaken, high latitudes will be overrepresented compared to lower latitudes in the AOD. Another benefit of our means of representation is that the impact of latitudinal transport can be tracked.

Specific comments:

Line 32: „an aerosol layer above 20 km altitude”

At mid and high latitudes the Junge layer is below 20 km.

That is the common description of the background aerosol serving as an introduction here. We have added: “in the tropics and at lower altitudes at high latitudes”.

Line 52: “inducing a global 1-year average effective radiative forcing of – 0.24 W/m2 .. “

Is there an error estimate for this forcing?

We have clarified the radiative forcing estimates in section 4.3 and removed the sentence you refer to here. See also response to reviewer 2.

Line 60: “On the other hand, SO2-poor eruptions ... is” -> “.. are”

Thank you

Line 83: “is divided into nine altitude and latitude parts”

In total or nine for each?

Thank you, we have clarified in the manuscript.

Line 87: “We find that the aerosol backscattering on average exceeded the background by 55% in the 17 years studied”

Considering all (i.e. averaged over all) altitude and latitude ranges?

Latitude-weighted (-80,80) in latitude. We have added the word global.

Line 91: "and 2019 (Raikoke eruption)."

Perhaps the Ulawun eruption also contributed? Ulawun (tropics) and Raikoke (NH mid-latitudes) both erupted in June 2019.

Yes, Ulawun contributed that year. We have clarified this.

Line 112: "and were first converted to extinction by the standard effective lidar ratio S = 50 Sr .."

It would be good to mention what particle size distribution (or at least what effective radius) this corresponds to. Particle size is quite variable in the stratosphere (with altitude, latitude and time).

Effects of particle size distribution (and particle shape and composition) are included in the lidar ratio.

Line 127: "the lowermost stratosphere (LMS, from the tropopause to the 380 K isentrope,"

Is the topopause always below the 380 K isentrope?

The tropopause is usually defined as the 380 K isentrope in the central tropics and is lower at higher altitudes.

Line 135: "To estimate the background conditions, the averages of the three years with the lowest backscattering measurements of each 8-day period were formed."

Not fully clear what was done here. Is the background an average over 3 years or an average over selected 8-day periods in these three years? This should be explained precisely.

We average the backscattering over 8-day periods. To estimate the background (or minimum) for each 8-day period, we form the average of the three years with lowest values. We have tried to formulate this clearer.

Line 140: "The extracted lowest 8-day values"

8-day values over 3 years?

See answer to line 135.

Line 140 following: The results of the background subtraction (including the background) are shown in Fig. 5, right? Please refer to this Figure here.

In a way that would make the explanations simpler, but according to the rules of this (and most) journal Fig. 5 would become Fig. 1, as that would be the figure that is first referred to in the running text. We do not wish to present a main result of the study in the methods section.

Line 157: "In that method a target value in scattering ratio (R) obtained beside the studied aerosol layer (RT) is reached below the layer in an iterative procedure that results in an estimate of the effective lidar ratio, while correcting for attenuation of the backscattered signal."

I have several comments/questions about this sentence. First, I don't understand its meaning, particularly of ".. a target value in scattering ratio (R) obtained beside the studied aerosol layer (RT) is reached below the layer". What does "beside" mean here? How is "layer" defined? Is it the entire aerosol layer or one of the three vertical layers you define? I think some essential information is missing for the reader to understand this sentence. It would also be good to describe the approach to determine the lidar ratio in a bit more detail here (a few sentences). If I understand this correctly the approach provides a height averaged lidar ratio? The ratio is highly variable with altitude. What are the effects of neglecting the altitude variation?

I think it should be mentioned explicitly that the lidar ratios are height averaged (in a probably non-trivial way).

We have added that "beside" means horizontally beside the investigated aerosol layer. It is correct that the effective lidar ratios obtained are affected by the entire aerosol layer, where the lidar ratio in principle could vary. We have clarified. More details on the method can be found in Martinsson et al. (2022).

Figure 2: What latitudes are analysed here? They are probably different for the different eruptions. What about the variation of the lidar ratio over longer periods? It usually takes about a month for the volcanic aerosol layer to "develop fully", in the sense that the SAOD reaches its maximum value.

When possible, we have studied the effective lidar ratio over the first month after the eruption as can be seen in Fig. 2. Most of the aerosol events show no systematic development in time. Exceptions are the 2017 Canada/USA wildfire and the 2022 Hunga Ha'apai eruption, where the first estimates after the event are somewhat higher than the later measurements.

Line 244: "formed large quantities of stratospheric aerosol (Martinsson et al., 2025)."

There are many early studies to back up this statement.

As far as we know, Martinsson et al. (2025) are the only to discuss the stratospheric aerosol in relation to "intense volcanism – sea interaction". That paper contains a large number of references to other studies that do not discuss volcanism – sea interaction.

Caption, Figure 3 (& several other Figure captions): "Aerosol scattering" is not precise enough. Please name the shown physical quantity correctly.

Same caption (also applicable to several other captions): "Color scale: Global AOD contribution per degree of latitude, i.e. the sum over latitude is the total AOD at any given time"

This is an unusual way to present results and in my opinion it is misleading. Every latitude bin has its own SAOD based on the vertical integral of the extinction coefficients over the corresponding altitude range. I suggest presenting the actual SAOD values, not rescaled ones. Their average then gives the global mean value. Your values are difficult to compare to other studies.

We have clearly defined what we show in the figure. This is a global study. If we do not correct for the surface area of a latitude band the high latitudes will have too strong influence related to the surface area of the earth that they cover. A benefit with latitude weighted AOD data is that it gives a more honest representation of the impact of latitudinal transport.

Line 277: "The LMS .. are affected" -> "the LMS .. is affected"

Thank you.

Line 285: "because of the time required to transform sulfur dioxide to sulfate," It's both, the conversion to sulfate and the microphysics, e.g. growth of existing or newly formed particles.

We have clarified that aerosol dynamics also could affect the evolution.

Line 288: "The aerosol of the latter eruption mainly consisted of volcanic ash (Vernier et al., 2013) and the former by aerosol from volcanism – sea interaction (Martinsson et al., 2025)."

The latter part of the sentence doesn't fit to the first part from a linguistic point of view.

Thank you, we have changed the wording.

Line 290: "These eruptions are thus less influenced by delay in aerosol formation from chemical transformation"

The main component of the Hunga aerosol layer in the stratosphere is also sulfate, right?

According to the estimates by Martinsson et al. (2025) SO_2 could only explain ~30% of the AOD.

Line 296: "Background aerosol backscattering"

Please use the correct technical term for this quantity.

Often the term "backscattering" is used throughout the paper. Please check all incidences and correct them.

The instrument measures backscattering. We cannot estimate the effective lidar ratio during background conditions because dS/dR is too large. Therefore, we discuss background in terms of backscattering before we discuss background extinction in comparisons made in section 4.4. We have clarified in the manuscript, see answer to your general comment 1.

Line 329: "using the average of the three lowest backscattering values"

What about years with strong volcanic perturbations? Are these years also used?

They are not close to background when affected by a strong aerosol event, and hence not used to estimate the background. In Figure 5 periods with strong signal is found above the orange background curve. We have rephrased in the methods section to clearer explain how we did.

Line 331: "Seven of the nine layers .."

I had to read this sentence several times to understand it. Perhaps you can add a first introductory sentence to this paragraph to set the stage.

We have tried to make this clearer.

Line 362: "The stratospheric background aerosol is often thought of as a layer located above 20 km altitude."

This is not generally assumed for high latitudes.

We have clarified according to the answer to "line 32".

Line 412: "The lidar ratios of the individual measurements are shown in Figure 2."

What altitude & latitude is shown in Fig. 2? What about the altitude variation in lidar ratio?

The latitudes are shown in Figure 7 d. We have not differentiated with respect to altitude. From working with the data: there was no obvious dependence on altitude. If present, altitude-dependence would have contributed the spread in Fig. 2.

Line 463: “The intense volcanism – sea interaction of the Hunga Ha’apai eruption in the beginning of 2022 (Martinsson et al., 2025)”

Again, there are dozens of earlier papers on this eruption.

See answer to “line 244”.

Line 482: “resulting in a variability range of 0.010 around the average of 0.0031”

Not really “around”, as this would also include negative values.

Thank you, we have changed the wording.

Line 482: “Making use of previous estimates of the relation between radiative forcing and stratospheric AOD”

Please mention how the conversion was done, i.e. what the conversion factor was.

We have added the information.

Line 491: “Comparisons of CALIOP lidar-based results with solar occultation (SAGE III/ISS) show discrepancy at mid- and high latitudes”

Discrepancies in which parameters and how large are the differences? Please describe briefly.

We have described the differences in more detail.

Line 500 following: for the AOD comparisons the reader needs to know the wavelengths of the data sets used (CALIOP and SAGE II/III). Or have the AODs been converted to the same wavelength? Also in this case the wavelength should be given.

Thank you, we have added the information.

Lie 519: “2 – 3 months after the Canada/USA fire (Table 1) similar deviations were found at high altitudes as in Kar et al. (2019),”

Please describe these discrepancies briefly semi-quantitatively. Right now, the reader cannot really interpret the statements.

We expanded the description at former line 491 and refer to that in former line 519.

Line 565: “Seven of the nine layers each contain 11- 15% of the entire background aerosol.”

With respect to mass? This is a difficult statement and I'm not sure it is correct.

With respect to backscattering. We have clarified the text.

Supplement: I think a bit of introductory text would be appropriate at the beginning of the supplement.

We have added a few lines.

Captions: “Aerosol scattering” is quite vague, as mentioned above. Also, the depiction of AOD contribution per degree latitude is difficult to interpret and to compare to other studies.

See answer to Caption Fig. 3.

Caption Fig. S7: "The sum of the nine AOD curves displayed is the average AOD from the tropopause to 35 km altitude in the latitude range -80 to 80°."

It is of course OK to sum up the three AODs for each latitude range to yield the total AOD for this latitude range, but the rescaling with latitude bins is not good in my opinion. How is this done in this case anyway? The tropical latitude range is narrower than the two other ranges?

See answer to Caption Fig. 3. The tropical range is narrower in terms of degrees but somewhat larger (6%) in terms of surface area.