Comments from Albert Ansmann

Thank you for your input. This is a comprehensive list of comments and questions. We hope that we have answered your questions satisfactory.

I studied the manuscript with large interest. To my opinion, it provides a very good overview of all the perturbations of the stratospheric aerosol layer around the globe during the last almost 20 years. And the paper comes on the market to the right time, now, in this era with increasing wildfire activities, and now, after completion of the CALIPSO mission.

However, during reading many questions came up, especially concerning the obtained column-integrated lidar ratio. Reviewer #1 mentioned already that more information and more comparison with the literature is needed here. So, I came to the conclusion, I should contribute to the discussion by writing my personal list of comments and recommendations.

Detailed list of comments:

Line 32: We observe stratospheric aerosol with a powerful lidar at Leipzig since 1995 and other groups (e.g., Sakai et al., JGR, 2025, https://doi.org/10.1029/2024JD041329, and Trickl et al., 2013, https://doi.org/10.5194/acp-13-5205-201323, Trickl et al,, 2024, https://doi.org/10.5194/acp-24-1997-2024) also do that. The stratosphere contains always some kind of background from the tropopause upward. But mention an aerosol layer above 20 km. Is the aerosol below 20 km then the result of downward mixing and sedimentation of aerosols from above 20 km? Should be clarified in the text.

Please see the answer to reviewer 2, line 32.

Line 41: Here, one could provide more wildfire-related references. We in Leipzig have strong focus on wildfire smoke since the Canadian Fire Event in August 2017: Baars et al., ACP, 2019, Haarig et al., ACP 2018, Ohneiser et al., ACP, 2020, 2021, 2022). There are further papers of Khaykin et al. (GRL, 2018, and Comm. Earth & Environ., 2020, https://doi.org/10.1038/s43247-020-00022-5) that could be cited.

We have added Ohneiser et al. (2020) in a lidar ratio comparison. In our previous papers dealing with the 2017 Canadian and 2019/20 Australian wildfires we refer to most of these important papers. Here our priorities are on overviewing the stratospheric volcanic, wildfire and background aerosol in the CALIOP era.

Line 54: Regarding radiation aspects there are many papers (e.g., Hirsch and Kohren, Science, 371, 1269–1274, 2021; Heinold et al, ACP, 2022) that contributed to the field.

We have removed the sentence and clarified in section 4.3, see also answer to reviewer 2, point 5.

Lines 89-93: Raikoke was stronger than Sarychev! SO_2 emissions were as high as 1.5 Mt SO_2 (Raikoke) and 1.2 Mt SO_2 (Sarychev). The literature shows maximum AOD values of 0.02 in the case of Sarychev and 0.025 (at 500-550nm) in the case of Raikoke.

According to our measurements, which we describe in that part of the manuscript (“This work…”) the maximum AOD were almost equal of the two eruptions from the tropopause to 35 km.

Lines 126-139: Our comparison with CALIOP observations (Ansmann et al., ACP 2023, MOSAiC paper) indicated a bias of 532nm AOD of 0.03 (CALIOP AOD values were too low by 0.03). This is also mentioned in one of the CALIOP papers cited in our paper. CALIOP does not detected very weak backscatter …adding up to an AOD bias of 0.03.

See answer to reviewer 1, point 1.

Lines 155-160: I checked the Martinsson et al. 2022 paper (already in 2022). I could imagine that multiple scattering may influence the retrieval. Furthermore, the reference values 'behind' the detected layer (close to the tropopause) are set to values varying around zero. This may too low! There is always some aerosol so that the backscatter is not zero, but larger. Then, the lidar ratio is the column-integrated lidar ratio. That means it is the backscatter-weighted mean lidar ratio of a given layer and not simply the layer mean value. The largest backscatter controls the lidar ratio result. Please have look into: Ansmann, Ground-truth aerosol lidar observations: can the Klett solutions obtained from ground and space be equal for the same aerosol case?, Appl. Opt, 2006, https://doi.org/10.1364/AO.45.003367 .

See our answer to your comments on Martinsson et al. (2022) on multiple scattering.

On "behind": Not all aerosol layers have a distinct lower boundary and can therefore not be investigated with the method we presented in Martinsson et al. (2022).

See also the answer to reviewer 3, line 157.

Line 172 and everywhere in the text: the lidar ratio should be given in sr, not in Sr (check wiki steradian).

You are right, we have changed.

Lines 209-232, and Figure 2: These values in the figure should be compared with available literature values. For smoke, there are several papers: Haarig et al, ACP, 2018, Ohneiser et al. 2020, 2022, Hu et al., ACP, 2019, https://doi.org/10.5194/acp-19-1173-2019. Haarig and Ohneiser observed much higher lidar ratios by using the classical Raman lidar method to obtain the lidar ratios. Hu et al. used a method similar to the Klett method to derive the stratospheric lidar ratios if I remember correctly. Their values appear to be too low.

For volcanic sulfate aerosol, one may check the computations of Jaeger and Deshler, GRL, 2003, doi:10.1029/2003GL017189. The lidar ratio should be low, below 30sr at 532nm, during the first month of the eruption. Our recent observations during the first half year of 2025 , about one year after the Ruang volcanic eruptions, yielded 50-60sr at 532 nm.

The Raikoko observations with CALIOP are not easy to interpret. We found that Siberian smoke as well as Raikoke aerosol polluted the stratosphere at least at latitudes greater than 65°N (Ansmann et al, JGR, 2024, comment paper, Ohneiser et al., ACP, 2021).

Herrore-Anta et al., ACP 2026 and Zhong et al , Nature comm., 2026, https://doi.org/10.1038/s41467-026-69728-y also conclude that smoke cannot be ignored.

Kloss et al., ACP, 2021, did not know about the Siberian smoke and thus did not consider that in their analysis.

This is a global overview of 17-year period. We have no intention to go into detail on minor events, see answer on background aerosol to reviewer 2, point 4.

Lines 269-270: I disagree that the perturbations rapidly faded! Baars et al., ACP, 2019, measured the wildfire smoke in the stratosphere over Europe even 6 months after the Canadian fire event in August 2017, and Ohneiser et al., ACP, 2022, measured a two-year long decay phase of the Australian fire perturbation in the stratosphere over southern Chile in 2020 and 2021. Chemical processing was probably not a big issue, because smoke particles are glassy at low temperatures in a dry atmosphere and probably chemically rather inactive.

See answer to reviewer 2, l.269. Most of the AOD was lost with a half-life of 10 days but the particle residuals remain in the stratosphere about a year (Martinsson et al., 2022). In this manuscript you can follow the influence of the particle residues in Figures 3, 5, 8, S1-S7.

Line 283: 2009 instead of 2006?

There was a small contribution to SH LMS in 2006 from the "great divides fire" in Australia (Table 1). The fire in Victoria, Australia in 2009 (mentioned in old line 271 of the manuscript) went deeper into the stratosphere.

Lines 292-294: So, if that is true how could we observe the smoke for more than 6 months (Canadian smoke) and 2 years (Australian smoke). Maybe, because the smoke particles are glassy at stratospheric conditions? The initial stratospheric smoke peaks cannot be used to describe the decay phases. These are signs of individual pyroCb events. The coherent decay phase starts about 2-3 weeks after the strong injection events.

You can still observe the residual particles after loss of the organic component of the aerosol as we described in Martinsson et al. (2022).

Section 4 and Figures 4 and 5: I strongly recommend to compare your time series (the integrated column backscatter values) with the ones presented by Sakai et al., JGR, 2025 and Trickl et al., 2013, 2024.

Please see our answer to your 2nd last comment below.

Figure 4: I estimate that the full stratospheric column background AOD is about 0.004 at 532 nm (assuming a lidar ratio of 50sr).

We get 0.0057 from the tropopause and 0.0039 above 15 km in a comparison with SAGE II data (line 509 in the original manuscript).

Then the Raikoko 532 nm AOD in the NH is roughly 0.01 – 0.004? when combining Figure 5 and 4 and assuming a lidar ratio of 50sr. This is quite low!

We cannot quite follow your reasoning here. In Figure 5 you see backscattering integrated over the vertical range and area weighted averaged over the latitudes in the form that the AOD is obtained by multiplication with the lidar ratio. Figure S7 shows the AOD of the same stratospheric parts.

And the CALIOP bias is about 0.03 as we pointed out in Ansmann et al. 2023?

Please clarify, many readers will have problems with these numbers. Please compare your findings with values presented by Sakai et al. (2025). Agreement with long term observations of Sakai et al. are very helpful in these discussions. Ground-based systems should be able to correctly measure the stratospheric column-integrated backscatter coefficient and therefore provide good estimates for the 532 nm AOD (even when simply assuming 50 sr for the lidar ratio).

See answer to reviewer 1, point 1 on the “bias”, which as far as we understand, refers to Caliop Levels 2&3, not level 1B.

On the numbers by Sakai et al. (2025): Indeed interesting, but we do not expect global averages from point measurements. The point comparisons with SAGE II are also interesting. Sakai et al. (2025) deal with the aerosol above 16.5 km. In our manuscript we find good agreement with SAGE II on the background AOD above 15 km. The large discrepancies between SAGE III/ISS and CALIOP reported by Kovilakam et al. (2023) are at altitudes below 17 km down to the tropopause. This is another interesting subject where ground-based lidar makes a great contribution.

Lines 470-480, and Figure 8: In Fig. 8, we have 532 nm AODs up to 0.01 and in the text we have AOD numbers of 0.06 (line 475), 0.05 (line 476) and 0.04 (line 477). Should that be 0.006, 0.005, 0.004? But all these values are quite low, to my opinion…?

You are right about the zeros missing, we have changed in the revision.

“…my opinion…”: We do not exactly know what you have in mind. If you think of values obtained in a latitude band, say 20 - 80° our values are approximately a factor of 3 lower because our data describe the average global impact. If you have global numbers in mind your numbers might include the background AOD of 0.0057. Our Figure 8 does not include the background aerosol, e.g., in Figure 8b the average AOD of 2013 is almost zero.