

We have reproduced the referee's comments below in black Times New Roman. Our responses are given in blue Tahoma.

The present manuscript deals with the calibration of the 1064 nm channel of CALIPSO. The calibration of the 1064 nm signal is done via the 532 nm signal. However, some corrections are necessary due to wavelength dependent signal attenuation in the stratosphere above the cirrus clouds. It once more shows the great care and effort by the CALIPSO team to provide the best possible data set from the CALIPSO mission. The scientific world can learn from their great expertise to solve the challenges of spaceborne lidar observations. The manuscript is clearly written and just needs minor revisions before publication.

Thank you very much for going through the paper carefully and for the helpful comments.

Major comments:

What is the highest ratio of the two-way transmissions recorded with SAGE-II and SAGE-III? Your color scale ends at 1.05, but maybe in extreme events higher ratios are possible, e.g., in the case of Pinatubo.

For Pinatubo era, the maximum value is ~ 1.20 . Note that the GloSSAC data we have used are zonally averaged—locally the values could be significantly higher, particularly immediately after the Pinatubo eruption. We have now changed the color scale saturating at 1.15 rather than 1.05 in Figure 1.

Could you also check ground-based lidar or AERONET observations of the AOD at these wavelengths in the stratosphere, if they provide higher values for some events? I agree, that higher ratios are not that common to be considered for the global observations of CALIPSO, but might occur.

This is an interesting idea, as it certainly seems plausible that isolated cases of higher T^2 ratios could (and most likely do?) occur. However, from a CALIOP calibration perspective, the presence of very high T^2 ratios is only a concern if/when our layer detection scheme fails to detect layer(s) responsible for these big ratios that also lie above 'calibration quality' cirrus. Assuming these upper layers are detected, cirrus below will never be considered as calibration candidates and hence will not be included when making the final calibration scale factor.

It also seems entirely plausible that we would encounter several difficulties when attempting to use ground-based measurements to characterize the upper end of the T^2 ratio distribution. For example, because AERONET is a total column measurement, stratospheric and tropospheric contributions to the AODs cannot be decoupled. Consequently, we cannot use AERONET to estimate stratospheric T^2 ratios without either (a) independent, collocated, multi-spectral measurements of either tropospheric or stratospheric AOD or (b) invoking some perhaps questionable assumptions. Lidars would be the sensor of choice for case (a), since the retrieved extinction profiles can be easily separated into tropospheric extinction below the tropopause and stratospheric extinction above. Unfortunately, retrieving extinction profiles from elastic backscatter lidar measurements requires the assumption of one or more fixed lidar ratios (e.g., one for the troposphere and one for the stratosphere), which now puts us squarely in case (b) territory (i.e., invoking perhaps questionable lidar ratio assumptions 😊). The ideal solution would be use multi-spectral Raman lidars or high spectral resolution lidars (HSRLs), as these systems can retrieve extinction without assuming a lidar ratio. But here too there are difficulties. To the best of our (admittedly imperfect) knowledge, there are only two 532+1064 Raman systems (Haarig et al., 2016; Wang et al., 2024) and only one HSRL system (Razenkova et al., 2023). (There are, of course, multiple systems of both types that make Raman/HSRL measurements at 355

nm and 532 nm. But 532 nm and 1064 nm systems are few and far between.) Once again, to the best of our (admittedly imperfect) knowledge, of the three candidate systems, only the TROPOS group has published stratospheric aerosol measurements at both 532 nm and 1064 nm. Haarig et al., 2018 report an Ångström exponent of 0.85 ± 0.03 for smoke from Canadian forest fires lofted into the lower stratosphere. For better or worse, this is the only reliable 532-to-1064 stratospheric aerosol Ångström exponent we know of. Haarig et al., 2018 also report that the 532 nm optical depth of the smoke layer varied between 0.2 and 1.0, implying T^2 ratios between 1.2 and 2.4. While T^2 ratios this large would be disastrous for CALIOP's 1064 nm calibration, we believe it unlikely that the CALIOP layer detection scheme would fail to detect such robust stratospheric layers.

To explore the span of T^2 ratios that might be encountered, the image below plots color-coded T^2 ratios as a function of Ångström exponent (y-axis) and 532 nm optical depth (x-axis). The solid purple line shows the $T^2_{1064} / T^2_{532} = 1.05$ contour.

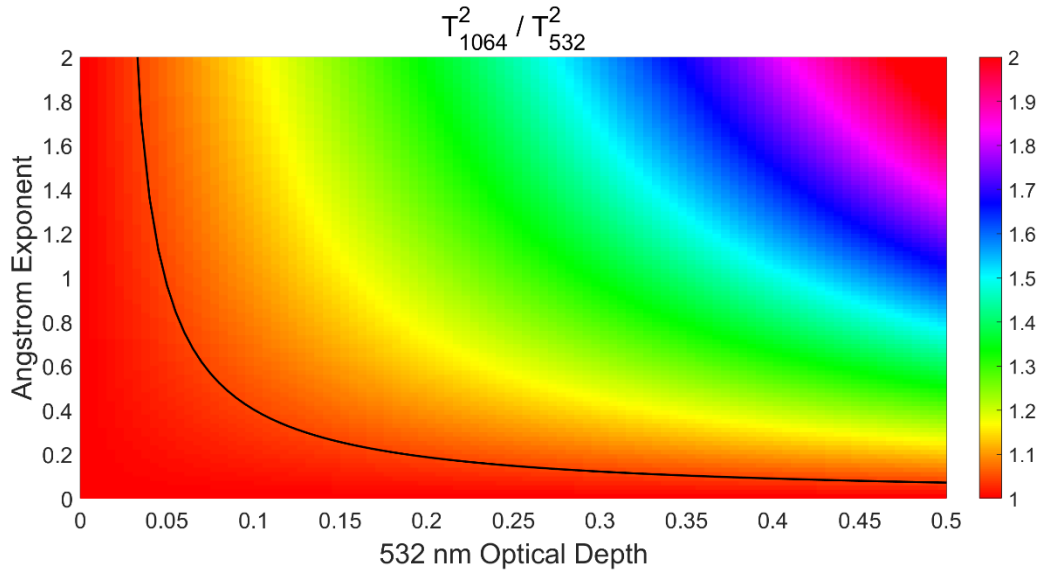


Figure 1: T^2 ratios as a function of Ångström exponent (y-axis) and 532 nm optical depth (x-axis). The solid purple line shows the $T^2_{1064} / T^2_{532} = 1.05$ contour. While the T^2 ratio color scale in this image saturates at 2, much higher values would be seen at larger optical depths.

For an Ångström exponent of 2, we reach the 1.05 contour line at an optical depth of ~ 0.04 , which is slightly above Ken Sassen's magic 0.03 optical depth threshold for subvisible cirrus. For CALIOP, successful detection of these optically thin layers depends very much on layer geometric thickness and lidar ratio: a geometrically thin layer with a low lidar ratio will be much easier to detect than a deep layer with a high lidar ratio. Diffuse, high lidar ratio layers (e.g., lofted smoke) could prove problematic for CALIOP's 1064 nm calibration technique.

The above comments are provided as background information only. To address the reviewer's comment in the manuscript we have added the italicized text below at the end of section 4.

"While only a tiny fraction ($\sim 0.3\%$) of the SAGE-measured T^2 ratios shown in Figure 4 exceed 1.05, it seems plausible that isolated cases of higher T^2 ratios could occur. One illustrative example is seen in data acquired by a Raman lidar in Leipzig, Germany operating at both 532 nm and 1064 nm (Haarig et al., 2018). Having multi-frequency Raman capabilities allows the Leipzig researchers to directly measure extinction coefficients at both wavelengths, and not have to rely on assumed fixed lidar ratios, as is done for elastic backscatter lidars (Winker et al., 2009). Using this system, Haarig et al., 2018 retrieved a 532 nm-to-1064 nm Ångström exponent of 0.85 ± 0.03 for an extensive smoke lofted into the lower stratosphere and transported from Canadian forest fires. The 532 nm optical depths

measured for this same layer varied considerably, from ~0.2 to ~1.0, implying T^2 ratios between 1.2 and 2.4; i.e., values well in excess of the maximum measured by SAGE. We note, however, that from a CALIOP calibration perspective, the presence of layers having unusually high T^2 ratios is only a concern if/when (a) these layers are not detected by the CALIOP layer detection algorithm and (b) 'calibration quality' cirrus clouds lie immediately below. As stated above, only the uppermost layer is considered in the calibration algorithm, and this upper layer must lie wholly below the local tropopause. Furthermore, because the CALIPSO 1064 nm calibration algorithm zonally averages multiple samples over a nominal 7-day temporal averaging window (Vaughan et al. 2019), occasional large localized T^2 ratios are unlikely to significantly alter the mean value of the calibration scale factor, f ."

(One final side note to this discussion: we contacted the Wisconsin group about their 1064 nm HSRL measurements and received this reply. "We do not have any direct measurements yet of aerosol Angstrom exponents from the new 1064 nm channel.")

Smoke layers lingering around the tropopause might not be included in stratospheric AOD. However, it was often observed that cirrus clouds form in these smoke layers. Recent studies using fluorescence lidars make these layers more visible, e.g., Gast et al., 2025 (and references therein).

As we mentioned in the introduction, particulates in both the upper troposphere and the stratosphere will impact the calibration at 1064 nm. We have presented the effect coming from the stratospheric loading only, using independent measurements. As for CALIPSO, one of the criteria for selecting a cirrus cloud for 1064 nm calibration is that it should be the uppermost "layer" detected up to 30 km. Vaughan et al. 2019, (their Fig. 7) presented an example of a smoke layer straddling the tropopause from the Black Saturday fire in Australia overlying a cirrus cloud. That cirrus cloud was not selected for calibration at 1064 nm because it was not the uppermost layer. Another criterion was that the cirrus cloud should be wholly below the tropopause (Vaughan et al., 2019). Therefore if the smoke layers lead to formation of cirrus cloud in the tropopause area they will be excluded. However future elastic space lidars may need to incorporate these issues in the 1064 nm calibration algorithm. Further, a tenuous layer detected by fluorescence and missed by the elastic signals (Gast et al., 2025), while important overall for characterizing the aerosol profile, may not contribute very much to the calibration issue.

Minor comments:

- A short outline of the article at the end of the introduction is common.—
Done.
- Generally, I would recommend to name the subfigures a, b, c ... for all figures..
Done.
- You mention future space missions operating an elastic lidar at 1064 nm. Already now, the Chinese ACDL is in space and operates at 1064 nm. Unfortunately, the data are not yet publicly available.
We agree with you. In particular, we could not find any publication detailing the ACDL 1064 nm calibration details.
- L179/180 Actually, under unperturbed conditions this ratio is everywhere around 1.0.
Yes, we have restructured the sentence as follows:

"As can be seen, this ratio mostly remains near 1.0 in unperturbed situations, similar to the SAGE II background conditions seen in the tropical regions (30°S-30°N) in Figure 1."

- L200: Actually, it is still debated whether a pyroCb event or self-lofting was responsible for the stratospheric smoke observed in Siberia.

We have replaced "pyroCb" by "wildfire" on line 200. As such the actual mechanism of injection of the smoke from Siberia is not important for our purpose.

- L219 Do I understand it correctly, that the described corrections will not be applied to the v5.0 data release, because the funding ends now?

Yes, that is correct. CALIPSO funding vanished forever on 30 September 2025. And the processing times required to successively extract level 1, level 2, and level 3 data products from ~17 years of a global, near continuous raw data stream are surprisingly long, even when using multi-processor, multi-cored cluster machines. Consequently, the CALIOP version 5.0 level 1 processing had already begun before we fully completed the analyses we report in this paper.

- L225-232 In Chapter 4, you're discussing the ratio of the two-way transmittance and from Chapter 5 onwards, you mostly speak about the calibration bias. A small sentence at the beginning of Chapter 5 would smooth the transition. Also, the true value of 1.02 mentioned in line 230 is just an assumed true value taken from the same location in Fig 5, which is actually for a different year and different month. I got a bit confused here and other readers might be as well.

We have added the sentence "In this section, we discuss the impact of this potential calibration bias on the downstream CALIPSO products." Further, there were no SAGE II or SAGE III data for the specific scene shown in Figure 7 and so we had to adopt the true value of the T^2 ratio from another year and month.

- Fig 8 + 9: May I suggest to add the perturbed extinction coefficient and attenuated backscatter coefficient to figures (left and center).

Please see the figures below. We've made the plots the reviewer suggested but are not especially impressed by the results. Certainly there's very little readily discernable information in the cirrus plot, as the extinction coefficients there are more than a factor of two larger than in the aerosol plot, making it difficult to appreciate changes on the order of 2 to 5 percent. There is arguably more to be gleaned from the aerosol plot, but the increase in relative difference with layer penetration depth is not at all obvious. (This decrease is an important take-away from this analysis that shows up nicely in the ratio plots) Because we believe that the ratio plots on the far right of figures 8 and 9 tell the story in a much clearer, more quantifiable way, we have chosen not to adopt the reviewer's suggestion for the revised manuscript.

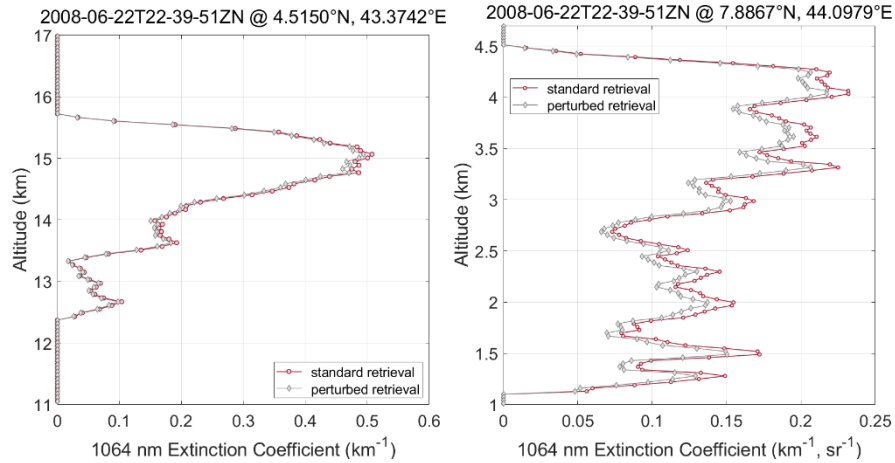


Figure 2: revised versions of the center panels of figure 8 (left) and figure 9 (right). In both cases, the original images of the standard retrievals alone have been augmented by adding plots (in grey) of the perturbed retrievals.

- Fig 10: I am just wondering how a zero-layer scene would look like as I assume that a planetary boundary layer should be always present. However, this question is not directly related to the findings presented in your study.

CALIOP reports “zero layer” scenes whenever the particulate loading falls below our minimum detectable backscatter (MDB). MDB is explained in the CALIOP layer detection ATBD. Toth et al., (2018) effectively translate MDB into optical depths via comparisons to MODIS.

- L367 The value of 1.05 is referred to the upper end of Fig 5+6, which display some months of the years of 2019 and 2020. Is it also the upper end of Fig 1+4 which report the SAGE-II and SAGE-III results? See also my major comment 1.

As mentioned above, we have now changed the scale for Figure 1, with upper end at 1.15 but have retained 1.05 as the upper end for Figures 4, 5 and 6.

Technical Corrections:

- Fig 5+6 The scale of the color bars is quite small. I would suggest to plot just one color bar for all 4 subplots.

Done.

- Fig 11 The date in the caption is wrong. It should be 16 June.

Thank you for pointing this out! It has been corrected.

- Fig 14 Again, the date in the figure caption is wrong. In the plots it is stated 2 January.

Thank you for pointing this out! It has been corrected.

- Fig 15 In the caption you want to refer to Fig 14 not 15. Furthermore, the calibration coefficient was increased by 5% (or by a factor of 1.05) and not 1.05%.

Thank you for pointing this out! It has been corrected.

References

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