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Comment on acp-2026-2575

Anonymous Referee #1

Referee comment on "Optical properties and global distribution of the Hunga aerosols 2022 observed by Aeolus and atmospheric lidars: new insights into the vertical sedimentation of stratospheric sulfate plumes" by Dimitri Trapon et al., Atmos. Chem. and Phys. Discuss., https://doi.org/10.5194/egusphere-2026-2575-RC1, 2026

Review of Trapon et al., 2026 - Optical properties and global distribution of the Hunga aerosols 2022 observed by Aeolus and atmospheric lidars: new insights into the vertical sedimentation of stratospheric sulfate plumes

Summary

This well written and detailed study by Trapon et al., describes the stratospheric sulfate plumes generated by the Hunga-Tonga volcanic eruption in January 2022 as observed, on a global scale, by the Aladin lidar onboard the ESA Aeolus satellite. Based on the optical properties derived from the lidar measurements, the authors analyse the zonal and meridional evolution of the aerosol plumes over a 6-week period stating 10 days after the eruption, highlighting diffusion, sedimentation and disaggregation processes of the plumes. They also provide indications on the microphysical properties of the particles composing the plumes.

The authors follow a rigorous path in their study, what strongly supports their findings and conclusions. Tracking of the stratospheric aerosols with single overpasses is first demonstrated and validated by comparing the Aladin observations with those performed by ground-based lidars (in La Réunion) and another space-borne lidar (CALIOP). Weekly maps of the aerosol optical properties are then produced and compared to quantities yielded from passive instruments onboard the Metop satellite (sulphate aerosols and SO4). Each step of the analysis is precisely detailed.

This paper is of importance for several reasons. From lidar measurement perspective, it shows that the Aladin UV HSL lidar instrument could characterise particles in the stratosphere, in particular of sub-micron size, and thus even if the instrument was originally designed for wind measurements. It allowed developing and testing algorithms and data processing tools, which are certainly useful for the present EarthCARE mission with the ATLID lidar onboard. Still from sub-micron size particle measurement perspective, Aladin bridges a gap between CALIOP and ATLID, an advantage for the continuity of lidar measurements from space. This manuscript also confirms several hypotheses previously made in other studies (based on in-situ or passive instruments) on the Hunga plumes evolution or on the size and growth process of the sulfate particles at different ages.

The referee enjoyed reading the manuscript paper and following its argumentation (although if quite dense sometime) and strongly support its publication in this proposed journal, which is adapted to the subject.

Specific comments

1) The manuscript deals with optical properties of particles in the atmosphere. The main referee's remark concerns the precision of the measurement products, or, said in other words, the uncertainties on the reported optical values.

The uncertainty aspect is tackled once, in lines 89-90 for the MLEsub products. The particle extinction coefficient α_{part} is said to by QC flagged valid if, among other, its standard deviations is below 1.0×10^{-2} m^{-1}. Similarly, β_{part} is flagged valid if, among other, its standard deviations is below 1.0×10^{-3} $m^{-1}sr^{-1}$. Reported values for α_{part} and β_{part} in Table 1, are (order of magnitude) at the level of 100 Mm^{-1} and 5 $Mm^{-1}sr^{-1}$, respectively, that is, 1.0×10^{-4} m^{-1} and 5.0×10^{-6} $m^{-1}sr^{-1}$. It would say that the error (standard deviation) is in both cases much larger than the reported value. Can the authors explain the referee where his/her thinking is wrong ? Are the indicated MLEsub standard deviation upper values given for one single laser shot ? Should they be understood in another way ? Completing the text accordingly is recommended, to avoid other readers to be in the same situation as the referee.

We thank the reviewer for pointing out this lack of clarity. The values mentioned in lines 89-90, i.e. 1.0×10^{-2} m^{-1} for the particle extinction coefficient α_{part} and 1.0×10^{-3} $m^{-1}sr^{-1}$ for the particle backscatter coefficient β_{part}, are the thresholds used in the Quality Check (QC) flags for MLEsub algorithm. In other words, these values correspond to the upper limits of standard deviations that are accepted per range bin. The scores with applied QC and indicated in bold in Table 1, i.e. 2.6 (6.8) - 2.6 (6.5) - 2.2 (4.5) for β_{part} and 96 (317.8) - 60.7 (232.3) - 33.6 (157.2) for α_{part}, are therefore computed from valid bins only, i.e. with lower error. Kindly note that these mean values are identical to the non-bold scores, meaning that very few bins were discarded in such conditions with MLEsub *SR* above 5.

In addition, we kindly remind that example of errors' order of magnitude are given in the study via Fig. 2b-c. The reported values for the lower altitude plume on 25 January 2022 where the centre of mass of the plume is identified (i.e. range bin from 18.04 km to 19.29 km in altitude) are the following : $\alpha_{part} = 0.090 \pm 0.074$ km^{-1} (i.e. $0.9 \pm 0.74 \times 10^{-4}$ m^{-1}), $\beta_{part} = 1.89 \pm 0.37 \times 10^{-3}$ $km^{-1}sr^{-1}$ (i.e. $1.89 \pm 0.37 \times 10^{-6}$ $m^{-1}sr^{-1}$). When comparing the standard deviations with the very QC upper limits (i.e. 0.74×10^{-4} m^{-1} with 1.0×10^{-2} m^{-1} for α_{part}, and 0.37×10^{-6} $m^{-1}sr^{-1}$ with 1.0×10^{-3} $m^{-1}sr^{-1}$ for β_{part}), this confirms that the errors for the in-core aerosol plume are much lower, then avoiding the flagging. The Fig. 2b-c also helps illustrating the fact that α_{part} is noiser and comes with higher errors if compared with β_{part}, as reported in Reitebuch et al., 2024 and Trapon et al., 2025.

We have modified the text in line 93, adding the errors for α_{part} and β_{part} in Mm^{-1} and $Mm^{-1}sr^{-1}$ respectively in parenthesis, i.e. $\alpha_{part} = 0.090 \pm 0.074$ km^{-1} (i.e. $90 \pm 74 \times Mm^{-1}$) and $\beta_{part} = 1.89 \pm 0.37 \times 10^{-3}$ $km^{-1}sr^{-1}$ (i.e. $1.89 \pm 0.37 \times Mm^{-1}sr^{-1}$). This also makes the comparison easier with the upper layers observed later on by 29 January 2022 and illustrated in section 3.1.1 with Fig. 2, as suggested in RC1 technical comment pointing to line 164 .

2) Section 4 (line 271). Where, in Section 3 on the results, can a SR of 40 be read or found? In particular in Figure 6, the color scale goes up to 14, what prevents from evidencing such a value. Can the authors explain a bit more the source of this value ? (The referee apologies if he/she missed something.) This value is claimed to be a "characteristic" property of the early SA patch and is cited in the abstract - and therefore, prone to propagate further in the literature. Consequently, it should be well supported. (Related extinction coefficient and LR values can be read easily from Figure A3.) The same remark applies to the LOD value of 0.2.

As mentioned by the referee, no mention of *SR* of 40 can be found in Section 3. This value comes from the handling of the dataset prepared for the early weekly global maps when checking the order of magnitude and adjusting the colorbar, which has finally set to a maximum of 14 to allow a better color contrast. Nevertheless, the Table 1 gives some mean and maximum values for overpasses (i.e. 29 January 2022, 31 January 2022, and 01 February 2022) which can be used as reference.

Therefore, it is proposed to replace the statement below in the abstract in lines 7-8 :

It exhibits high UV signal extinction up to ≈ 350 Mm^{-1}, scattering ratio (SR) up to ≈ 40, local optical depth (LOD) above 0.2, and lidar ratio (LR) above 100.

by :

It exhibits high UV signal extinction up to ≈ 318 Mm^{-1}, scattering ratio (SR) up to ≈ 37, local optical depth (LOD) up to ≈ 0.4, and lidar ratio (LR) up to ≈ 135 sr.

Similar values have been used as reference in the Discussions and conclusion section.

Other specific comments

3) Line 80: It would be worth remembering to the reader that, due to instrument design, only co-polarized signals are measured with ALADIN - particularly in view of Section 3.1.2, where ALADIN and CALIPSO results are compared.

CORRECTED. The introduction has been rephrased and the description of the Aeolus limitation, i.e. measuring only the co-polarized component of the backscattered light (parallel to the laser beam), has been added to the section 2.1.

4) Line 140. "[...] signal accumulation was done between ≈ 02:00 and ≈ 15:00 UTC". Not clear. Shall it be understood that the (night-time, according to Baron et al, 2023) ground-based lidar measurement started on 25 Jan. 15:00 UTC and finished on 26 Jan. 02:00 UTC ? Please, explain more clearly.

We thank the referee for pointing out this misunderstanding. The signal accumulation of the OPAR Maïdo lidars are routinely done two nights per week from 7 pm to 1 am local time (Gantois et al., 2024). During the first week after Hunga eruption, extended measurements were performed during the whole night: from 14:44 UTC on 25 January until 01:29 UTC on 26 January. The text has been corrected accordingly in section 3.1.1 in lines 148-150 and in section A1.1 in lines 526, 527.

5) Line 269. As shown in Fig. A1b,d, a two-week Earth circumnavigation was observed. Are there evidences that such a phenomena was observed several times ? Shoudn't the word "every" be replaced by "in" ?
The two-week Earth circumnavigation of the sulfate plume shown in Fig. A1b,d (e.g. light yellow to red color codes between 15° S and 25° S) can be observed repeating two weeks later, i.e. in Fig. A1d,f. Nevertheless we agree that the color contrast makes this second Earth circumnavigation hardly distinguishable for the readers. It is proposed to replace the word "every" by "in" as recommended.

Technical comments

- Line 45. Rather "40 sr ± 20 sr", or "(40 ± 20) sr" than "40 sr ± 20".
  CORRECTED, with format "40 ± 20 sr" usually recommended by Copernicus journals.

- Legend of Figure 1: Indicate what OPAR is (-> Observatory of Atmospheric Physics in Reunion), as this acronym was not met before.
  CORRECTED

- Line 136. It should be made clear in the main text what Li1200 and LiO3S are - there is not even an indication to refer to Section A1.1 (line 70 mentions link to datasets, not to instruments). Please, add some words mentioning they are ground-based lidar instruments at OPAR, with details given in Annex A.
  CORRECTED

- Line 153. Probably reference to Fig. 3 and not to Fig. 4.
  The reference to Fig. 3 with CALIOP $\beta_{tot,532}^{att\,\parallel}$ is correct, but we agree that re-phrasing might help. It is proposed to add "CALIOP" before $\beta_{tot,532}^{att\,\parallel}$ in line 154. Similarly, it is proposed to add "Aeolus SCA total attenuated backscatter" before β_{tot}^{att} in line 150.

- Figure 3, CALIOP plots. Why the color bar label mentions "perpendicular" and not "parallel" or "co-polar", while the plot title makes reference to "parallel" (//) ?
  CORRECTED. It was initially decided to use the mention "Total - perpendicular" to emphasize the derivation of CALIOP parallel-only 532 nm attenuated backscatter from the signals, as explained in Appendix A1.3 in line 538. It is indeed not a ready-to-use product. But we agree with the referee that the mention "Total - perpendicular" may be replaced by "Parallel" in Fig. 3 for consistency. The correction has been made.

- Line 159. The values given for *SR* and a_{part} concern Jan. 29 according to Table 1; this contradicts what is mentioned on the previous line. The text for lines 158 and 162 shall be reordered.
  CORRECTED. The text for lines 158 to 162 has been rephrased as follows : "If compared to the averaged scores, the layers on 29 January 2022, 31 January 2022, and 01 February 2022 exhibit high MLEsub maximum values of *SR* and α_{part}, i.e. 37.1 and 317.8, 21.9 and 232.3, 15.6 and 157.2 respectively".

- Line 164. Maybe the reader can be supported in his/her comparison by mentioning that the values above La Reunion were lower (by a factor of ~2 to 3 for the max. values).
  CORRECTED. We thank the referee for the suggestion. The text in line 164 has been modified as follows : “It is also important to note the deviation of these optical properties when compared to the early plume on 25 January 2022 observed in altitude ≈ 19 km and illustrated in Fig.2 : the maximum values for MLEsub α_{part}, β_{part} are higher by a factor of ≈ 2 to 3 and the maximum values for MLEsub *SR* are higher by a factor of ≈ 5 to 11”.

- Line 188. Separation between two words is missing.
  CORRECTED

- Line 188. Northern East branch altitudes are said to extend from ~18 km to 22 km. From the color code, one might rather say from ~20 km to 22 km.
  CORRECTED, in lines 199 and 207. We inform the referee that further testing of colorscale have been conducted. As a result, the MATLAB linear colorscale « turbo » in Fig. 4 has been replaced by a more discrete versions with segmented color blocks avoiding light yellow in order to improve the color contrast for the main height regimes described in the text. The new colorscale helps to visualize the northern East branch extent from ~ 19 km to 22 km. This is also coherent with the value of 19.3 km reported in Baron et al. (2023) and reported in section 3.1.1. It was then decided to modify the text accordingly in the abstract in line 13, in section 3.2.1 in line 208, and within the Discussion and conclusion in line 276.

- Line 193. Extra word, either “the” or “these”.
  CORRECTED, with “these”.

- Line 201. Repetition of word “the”.
  CORRECTED

- Figure 4d. For consistency with Figure A2, it is advised to use the same color limits, i.e., 0 to 2.0 [ppbv] and not 0 to 2.5 [ppbv]. (This rule is applied to Figure 4c.)
  CORRECTED. The color limits 0 to 2.5 [ppbv] has been applied to Fig. A2. In addition, the legend font-size for Figs. A2-A3 have been slightly decreased.

- Line 226. Extra word “in” ?
  CORRECTED. Yes, the extra word “in” has been removed.

- Line 226. For improved readability, it would be better to remplace “[…] mid February 2022 and Figure 7a-f [,,,]” by “[…] mid February 2022; Figure 7a-f […]”.
  CORRECTED as suggested.

- Line 228. Space missing in “FigureA2a-f”.$
  CORRECTED

- Line 230. Not fully understandable. Possible improvement: remplace “[…] (2024b), and it’s trace in Fig. 7a-c (i.e. light blue) with in altitude below ≈ 22 km in altitude indeed disappears, […]” by “[…] (2024b); its trace in Fig. 7a-c (i.e. light blue) with altitude below ≈ 22 km indeed disappears, […]”
  CORRECTED as suggested.

- Line 232. “SA patches come” instead of “SA patches comes”.
  CORRECTED

  Line 244. Unit of the aerosol effective radius: most probably [mm] instead of [nm].
  CORRECTED. We thank the referee for pointing out this inconsistency. It is proposed to replace the text “...two peaks of ≈ 0.3–0.4 nm can be respectively observed at distinct altitudes ≈ 24 and ≈ 26 km by February 2022.” by “...two peaks of ≈ 325–425 nm can be respectively observed at distinct altitudes ≈ 24 and ≈ 26 km by February 2022 for the latitudes band [15° S– 25° S].”. This makes the comparison with Fig. 3.3a of Khaykin and Bourassa (2025) easier.

- Line 256. Extra word “in” ?
  CORRECTED. Yes, the extra word “in” has been removed.

  Line 263. It would be convenient to indicate some values for the IMS SA OD and the IMS SO2 concentration, for comparison with values provided in Line 267.
  CORRECTED as suggested. The text has been modified as follows : “The map reveal a short-lived plume with low IMS SA OD (i.e. below 0.015) and high IMS SO2 concentration (i.e. up to ≈ 2.0 [ppbv]) above south Africa...”

- Line 264. Location and latitude range should match those mentioned in Line 199 (or vice versa) for consistency.
  CORRECTED as suggested in lines 207-210, i.e. aligned with [25° S–10° S] and 19 to 22 km.

- Line 271. “stabilizes” instead of “stabilize”.
  CORRECTED

- Line 275. Extra “)”.
  CORRECTED

- Line 286. “argues” instead of “argue”.
  CORRECTED

- Figure A5. The first line of the legend contains the words “QC flagged”, while they are absent in the legends of Figures A3 and A4, Why ? Is there actually a difference ?
  CORRECTED. We thank the referee for pointing out this inconsistency. The words “QC flagged” have been removed from the first line of Fig. A5 legend. We kindly remind that the QC are applied for all two-dimensional histograms, as mentioned in the second lines of Figs. A3-A4-A5 legends.