**Interactive comment on "Recovery of the first ever multi-year lidar dataset of the stratospheric aerosol layer, from Lexington, MA, and Fairbanks, AK, January 1964 to July 1965" by**

**Juan-Carlos Antuña-Marrero et al.**

**Replies to Reviewers Comments:**

We thank the reviewers for the comments and suggestions they made, contributing to the improvement of the manuscript.

**Introductory statement by the authors:**

The SSIRC data rescue activity has a philosophy to involve modeling scientists as well as observational scientists, both to improve communication between the often separated communities, and also to help identify priority measurement datasets and aerosol metrics that can be of most benefit to the modelling community. Reviewer 1 has requested to remove the comparisons to model predictions dataset in section 3.4 of the manuscript, but we feel strongly this is an important element of the manuscript, highlighting why the observations dataset is of such importance both to current international climate modelling activities such as CMIP6 (Eyring et al., 2016; Zanchettin et al., 2016) and to stratospheric aerosol modelling activities such as ISA-MIP (Timmreck et al. 2018). We feel that applying strict rules to separate the publication of observational datasets and modelling datasets would in this case be in confict also with the spirit of the ESSD journal to promote international interdisciplinary research.

**Anonymous Referee #2**

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This paper presents in my view a valuable contribution to the rescue of old geophysical data - in this case from early lidar measurements of stratospheric aerosols - for the sake of their use in the reconstruction of past volcanic events. The work is a contribution to the Data Rescue activity of the Stratospheric Sulfur and its Role in Climate within the SPARC project.

The crux of the work is the extraction of aerosol extinction coefficients at 532 nm between 12 km and 24 km from backscattering-ratio results at 694 nm retrieved in that range, under simplifying hypotheses, from lidar measurements carried out in different periods of 1964 at two different locations (Lexington, Massachusetts, and College, Alaska).

While the "translation" from the original results (the backscattering ratios at a given wavelength under the mentioned simplifying hypotheses) to the extracted ones (the aerosol extinction coefficients at another wavelength and correcting for the simplifications) is carefully explained, I found apparent inconsistences and ambiguities in the developed formulation, as well as in the notation, that the authors should explain or, if my concerns are proven right, correct.

A revision of the English writing and a more direct style, with less involved sentences, would probably be beneficial as well.

See attached pdf for review details.

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**Review of ESSD-2020-246**

This paper presents in my view a valuable contribution to the rescue of old geophysical data - in this case from early lidar measurements of stratospheric aerosols - for the sake of their use in the reconstruction of past volcanic events. The work is a contribution to the Data Rescue activity of the Stratospheric Sulfur and its Role in Climate within the SPARC project.

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While the "translation" from the original results (the backscattering ratios at a given wavelength under the mentioned simplifying hypotheses) to the extracted ones (the aerosol extinction coefficients at another wavelength and correcting for the simplifications) is carefully explained, I found apparent inconsistences and ambiguities in the developed formulation, as well as in the notation, that the authors should explain or, if my concerns are proven right, correct.

**I. The notation ambiguities are the following:**

1. The symbol $N_A$ is used with two different meanings. In Eq. (1) $N_A(z)$ is the molecular number density at altitude z. In Eq. (4) it is used for Avogadro's number.

**Reply:** We agree with the reviewer. We made the pertinent corrections: $N_A$ is only used for Avogadro's number and $N_d(z)$ for the molecular number density at altitude z.

2. Two different symbols, $SR_o(\lambda,z)$ and $SR(\lambda,z)$, are used for the backscattering ratio without a clear reason for establishing a difference. This is apparent in Eq. (6), where the $\beta_a^A(z)$ expression is said to be derived from Eq. (2), but the symbol $SR_o(\lambda,z)$ appears instead of the symbol $SR(\lambda,z)$ used in Eq. (2).

**Reply:** We agree with the reviewer. The two different symbols SRo(λ,z) and SR(λ,z) could be really confusing. They were used with the purpose to differentiate the derived SR at 694 nm to the general SR at any wavelength used in the algorithm description. We replaced SRo(λ,z) by SR(694, z) in the manuscript.

In addition the first sentence in line 144 was modified. It reads now:

*"We first describe the procedure applied in G-66 to derive the backscattering ratio (SR(694, z)).".*

We also corrected Eq. (6) accordingly. It is now: $\beta_a^A(\lambda,z) = (SR(694,z) - 1)\,\beta_m(\lambda,z)$

3. The notation $T_{2w}^2$ for the two-way atmospheric transmittance is in my view redundant: the two-way is implicit in the squared superscript. The subscript should be left to denote the origin of the transmittance, as is generically done in Eq. (8). Note, related to this, that in line 180 $T_T^2$ seems to be used with the same meaning as $T_{2w}^2$.

**Reply:** We agree with the reviewer. We went further and simplified the symbol used for the two way transmittance eliminating the squared superscript but retaining the subscript to denote the origin of the transmittance.

In addition in section *"2.7.1 Backscattering ratio relative error"* we replaced $T_{2w}$ by $T_T$ to be consistent with the definition of two-way total transmittance.

4. Letting aside possible corrections arising from the reasoning in point #2 sin section III of this review below, on possible inconsistences in the formulation, I think that there are possible ambiguities in the notation that should be clarified. For example, $\beta_a^A(z)$ is used to denote the aerosol attenuated backscatter coefficient, which is subsequently corrected by the molecular and ozone transmittances in Eq. (7). The result is called $\beta_a$ which seems to imply that this is the final aerosol backscatter coefficient, from which using the factor BEc(z, t) (by the way, what does the variable t mean?) the aerosol extinction coefficient $\alpha_a$ is derived. (Eq. (9)). But then one discovers that $\alpha_a$ has still to be corrected for the aerosol transmittance, the final aerosol extinction coefficient being called $\alpha_a^{Ta}$. Although this is a minor remark, I think it would be clearer to reserve the symbols $\beta_a^{Ta}$ , $\alpha_a^{Ta}$ for the coefficients yet to be corrected for the aerosol transmittance and to use $\beta_a$ , $\alpha_a$ for the final, fully corrected extinction coefficient.

**Reply:** We do not agree with the reviewer. In the early times after the first lidars were operative the application of the two-way transmittance correction for the processing of stratospheric aerosol lidar returns was commonly neglected. The use of the symbols $\beta_a^{T_a}$ , $\alpha_a^{T_a}$ for the two-way transmittance corrected aerosol backscatter and aerosol extinction has the purpose to highlight the application of this correction.

Taking into account the reviewer comment on the meaning of variable t in the backscattering to extinction conversion coefficients from λ = 694 nm to λ = 532 nm (BEc(z, t)) we added *"altitude and time dependent"* on line 207 it, then that sentence is now:

*"…where BEc(z, t) are the altitude and time dependent backscattering to extinction conversion coefficients from λ = 694 nm to λ = 532 nm also derived for the Mt Pinatubo (Jäger and Deshler, 2003).*

**II. With respect to the inconsistences in the formul developments:**

1. While Eq. (1) is actually found in ref. **G-66** (Eq. (4.2) of this reference), it should be noted that this equation refers to the "expected signal from a molecular atmosphere" (page 50 of **G-66**), as it is made clear by the sentence (also in page 50 of **G-66**): "Thus, to derive the dust profiles, it is necessary to evaluate the intensity of the echoes for a dust-free atmosphere by using equation 3.8 for the case of Rayleigh scattering by air molecules."
Therefore the statement on line 144 of the paper under review referring to Eq, (1): "The average photoelectron flux registered by the photomultiplier was represented by the expression:" is misleading: the average photoelectron flux was actually represented by $\frac{\mathrm{dn(z)}}{\mathrm{dt}} = \mathrm{K}\frac{\beta}{\mathrm{z}^2}$ (Eq. (3.8) of **G-66**, with K condensing all the multiplicative constants in that equation), Eq. (1) corresponding to the expected photoelectron flux from a molecular (reference) atmosphere.

**Reply:** The reviewer is right, we made a mistake. Corrected and added an explicit reference to the Eq. in **G-66**.

2. The authors seem to imply that the scattering ratio SRo(λ,z) obtained in ref. **G-66** corresponds to the expression in Eq. (2). However, following the data processing steps described in the paper I don't arrive at that expression. I explain in detail my understanding of the steps described in the manuscript to sustain my statement. For clarity, I keep the authors' notation notwithstanding my remark #3 on the notation in section I above:
a) To begin with, the photoelectron flux will actually be, taking into account the atmospheric transmittance,

$$\frac{dn(z)}{dt} = K\frac{\beta(\lambda,z)T_{2w}^2(z)}{z^2} = K\frac{\left[\beta_m(\lambda,z)+\beta_a(\lambda,z)\right]T_{2w}^2(z)}{z^2},$$

with $\beta(\lambda,z) = \beta_m(\lambda,z) + \beta_a(\lambda,z)$ the total backscatter coefficient, being $\beta_m(\lambda,z)$ and $\beta_a(\lambda,z)$ the molecular and aerosol backscatter coefficients respectively. Note that I'm using $\beta_a(\lambda,z)$ for the "true" (i.e. not affected by any attenuation) aerosol backscatter coefficient.
b) According to the authors: "Next, the ratios between the averaged signal at each level and the values at the same level of the right side of the equation (1) were calculated for each profile between 12 and 30 km". Calling that ratio $P_n$, I understand that

$$P_n(z) = \frac{\frac{dn(z)}{dt}}{\frac{N_A(z)}{z^2}} = K\frac{\left[\beta_m(\lambda,z)+\beta_a(\lambda,z)\right]T_{2w}^2(z)}{\beta_m(\lambda,z)}$$

c) Following the authors: "A final step consisted in normalizing the ratios calculated in each profile between 12 and 24 km. To that end, for each profile the average value between 25 and 30 km of the ratios calculated in the former step were determined". I will call the average value between 25 and

30 km $\overline{P}_n(z_1, z_2)$, with $z_1$ = 25 km and $z_2$ = 30 km. Then, assuming, as **G-66** seem to do, that $\beta_a(\lambda, z)$ is negligible in that range, we would have

$$\overline{P}_n(z_1, z_2) = \frac{1}{z_2 - z_1}\int_{z_1}^{z_2} P_n(z)\,dz = \frac{K}{z_2 - z_1}\int_{z_1}^{z_2} T_{2w}^2(z)\,dz = KT_{2w}^2(z_0),$$

with, according to the finite-increment theorem, $z_0$ a range between $z_1$ and $z_2$ whose value will depend on the form of $T_{2w}^2(z)$

d) Always following the authors: "Then for each profile the ratios in the altitude range 12 and 24 km were divided by the average value of the ratios between 25 and 30 km from the same profile". Calling, as the authors seem to do, SR(λ,z) (or $SR_0(\lambda,z)$, see my remark #2 on the notation ambiguities) the result of dividing the ratio $P_n$. in the range 12 km – 24 km by $\overline{P}_n(z_1, z_2)$. Then

$$SR(\lambda, z) = \frac{P_n(z)}{\overline{P}_n(z_1, z_2)} = \frac{\left[\beta_m(\lambda, z) + \beta_a(\lambda, z)\right] T_{2w}^2(z)}{\beta_m(\lambda, z) T_{2w}^2(z_0)},$$

which do not coincide with the result given in Eq. (2).

The authors should, either point out possible mistakes I may have committed in the above development, or else correct theirs.

**Reply**: The derivation conducted by the reviewed is right, no mistakes. The 2 two-way transmission terms are part or the exact definition of SR(λ,z) with no assumptions about the two-way transmission. However, the sentence before Eq. (2) states:

> *"That definition is associated to the fact that in the retrieval of SRo (z) the two-way total transmittance $T_T^2(z)$ correction was neglected (Hostetler et al., 2006)".*

The neglected two-way transmittance implies $T_{2w}^2(z) = 1$, ending in the formulation in Eq. (2). We took into account the fact that in **G-66** SRo(694,z) was derived with similar assumption.

4. I couldn't understand the (iterative?) process described in lines 211 – 217 to obtain $\alpha_a^{Ta}$. How is the $T_a^2(532, z)$ first guess of obtained? How is it refined? I suggest illustrating the procedure with a graph.

**Reply**: We agree with the reviewer the way it has been described is complicated to understand. We rewrote it. It is now:

> *"Because the information available to calculate $T_a(532, z)$ should be determined using the total aerosol optical depth (TAOD) measurements from sun photometers we applied a two-step procedure. The first step consists of using the TAOD to calculate a first guess $T_a(532)$, which is a unique value for all the altitudes. It is follow by calculation of a first guess $\alpha_a^{Ta}(532, z)_*$ profile. Then the stratospheric AOD (sAOD) is calculated integrating $\alpha_a^{Ta}(532, z)_*$ between 12 and 24 km. The second step calculates (see Suplement-1 for details on the calculations of TAOD):*
>
> $$tAOD = TAOD - sAOD \qquad (11)$$
>
> *producing a profile of $T_a(532, z)$ with the particularity of having a constant value of $T_a(532)$ from the surface to 11 km and then a profile of $T_a(532, z)$ between 12 and 24 km. This profile of $T_a(532, z)$ is applied in equation (11) getting the definitive values of $\alpha_a^{Ta}(532, z)$."*

**III. Other remarks**

1. Line 77: "were been produce" should be just "were produced".

**Reply**: Corrected

2. Line 80: "Giogio Fiocco" should be "Giorgio Fiocco".

**Reply**: Corrected

3. Line 86: What do CMIP5 and CMIP6 refer to? Further explanations and possibly a reference are needed.

**Reply**: Defined CMIP5 and CMIP6 (Coupled Model Intercomparison Projects 5 and 6) and the respective references added:

Taylor, K. E., Stouffer, R. J., and Meehl, G. A.: An overview of CMIP5 and the experiment design, B. Am. Meteorol. Soc., 93, 485–498, 2012.

Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, Geosci. Model Dev., 9, 1937-1958, https://doi.org/10.5194/gmd-9-1937-2016, 2016.

Zanchettin, D., Khodri M, Timmreck C, Toohey M, Schmidt A, Gerber EP, Hegerl G, Robock A, Pausata FSR, Ball WT, Bauer SE, Bekki S, Dhomse SS, LeGrande AN, Mann, GW, Marshall L, Mills M, Marchand M, Niemeier U, Poulain V, Rozanov E, Rubino A, Stenke A, Tsigaridis K, Tummon F., The Model Intercomparison Project on the climatic response to volcanic forcing (VolMIP): experimental design and forcing input data for CMIP6. *Geosci. Mod. Dev*., **9**(8), 2701-2719, https://doi.org/10.5194/gmd-9-2701-2016, 2016.

4. Line 137: "It is well known that solving the equation for the single wavelength elastic lidar is an ill-posed problem". What is the equation the sentence refers to? I suppose it is the lidar equation, so the sentence should read "It is well known that solving the lidar equation…" As implied in remark # 1 of section II of this review, this should be Eq. (1), but written in the form of Eq. (3.8) of G-66.

**Reply**: The reviewer is right. Corrected.

5. Line 105: the description "very high frequency nano-second laser" is misleading, as it seems to mix the laser pulse duration with the pulse repetition rate, the latter being very low, according to table 1, by today's standards.

**Reply**: Corrected. Replaced "very high frequency nano-second laser" by "a short pulse (micro-second)".

6. I think it would be beneficial for the reader to point out that $\frac{dn(z)}{dt}$ is proportional to the collected backscattered power from that range z.

**Reply**: We agree with the reviewer suggestion. Added.

7. Line 146: $n_z$ should probably be n(z).

**Reply**: The reviewer is correct. Corrected.

8. The references Jäger and Deshler, 2002 and 2003 are missing in the reference list. Please check that all cited references are included in the list.

**Reply**: Corrected.

9. Line 181: "Hosteler" should be "Hostetler"

**Reply**: Corrected.

10. In lines 186-187 it is said: "after neglecting the dispersion of the refractive index and the King factor of the air represented by kbw". If the King factor is not used, it is unnecessary to assign it a symbol.

**Reply**: WE are not assigning the King Factor a symbol. The expression Sm = (8π/3)kbw defining the molecular extinction to backscatter ratio on line 186, contains it, what we do is defining a symbol we used.

11. Unnecessary bracketing is used sometimes. For example, but not only, Eq. (2) could simply be written as

$$SR(\lambda, z) = \frac{\beta_m(\lambda, z) + \beta_a^A(\lambda, z)}{\beta_m(\lambda, z)},$$

without the curly brackets. Other instances of unnecessary brackets are found throughout the paper.

**Reply**: Corrected. Eliminated unnecessary bracketing in equations 2, 15 and 17

12. Lines 321-322: "After the conversion to 532 nm they were respectively 0.087 and 0.242…" What does "they" in this sentence refers to?

**Reply**: The section containing the text has been moved to the Supplement 1 (attached). There the sentence has been modified. It is now:

*"After the conversion to 532 nm they were respectively 0.087 from Fairbanks, AK, in July 1978 (Shaw, 1982) and 0.242 from the July 1997- 2019 climatology at Bonanza Creek, AK. We used the contemporary July 1978 value for both July and August 1964 lidar measurements at Fairbanks."*

13. Line 326: "The high TAOD values for the twos series…" Which ones are the two series?

**Reply**: The section containing the text has been moved to the Supplement 1 (attached). There the sentence has been modified. It is now:

*"The high TAOD values of the two series, from the Blue Hill Observatory (1961-1966) and the Eastern US (1972-1975), are representative of what have been reported for the Eastern US (Husar et al., 1991)."*

14. Lines 367-368: "the data we rescue are a reasonable approximation of what we today know as the backscattering ratio described in equation (2)". But the definition in Eq. (2), letting aside the concerns expressed in point #2 of section II of this review as to its derivation, uses the attenuated backscatter, while, in my understanding, the present-day backscattering ratio definition is $SR = \frac{\beta_m + \beta_a}{\beta_m}$,

with $\beta_m$ the molecular backscatter coefficient and a $\beta_a$ the "true", not affected by any attenuation, aerosol backscatter coefficient.

**Reply**: We agree with the reviewer. We eliminated that the sentence.

**Other corrections:**

1. The Equation on line 444 was incorrectly numbered (18) while there was jump on the numbering jump equation (17) to (19). The numbers were reassigned after the number (17) in the order they were listed in the manuscript. A new re-assignation of equations numbers was made from equation 14 on, after the transfer of section 2.5 to the Supplement 1 eliminated equations (12) and (13) from the manuscript.

2. The decision to move the subsections in section 2.5 to Supplement 1 included eliminating figure 1 and table 2 from the manuscript. Consequently all the figures and tables were re-numbered. Also several references not cited in the new version of the manuscript were erased.

3. The term "*cross section*" was replaced by "*contours*" in the manuscript.

4. Several sentences have been rewritten:

**Lines 134-135:** The sentences

*"The lidar signal returns at both sites were registered photographically from oscilloscopes covering up to 40 km and then digitized. Then the digitized lidar return signals from a set of daily laser shots were averaged in 1 km bins (G-66; GF-67)."*

Were replaced by:

*"A single laser shot was registered by photographing the contribution of daylight return signal on an oscilloscope covering up to the40 km, and then digitized by hand. The digitized return signals registered by the photomultipliers from a set of laser shots were then averaged in 1 km bins (G-66; GF-67)."*

**Line 231-233:** The sentence:

*"There have been abundant accounts about the changes of the physical-chemical properties of aerosols in the eastern US from the sixties until the present (Went, 1960; Husar et al., 1991)."*

Was replaced by:

*"Changes of the aerosols physical and chemical properties from the sixties until the present in the eastern US has been documented (Went, 1960; Husar et al., 1991)."*

**Line 380-381:** The sentence:

*“It is a plausible assumption because the profile βm used the US 1962 Standard Atmosphere for the vertical resolution of the lidar.”*

I rewrote it:

*“The use at the lidar levels of interpolated β_m values from the lower resolution ones calculated using the US 1962 Standard Atmosphere, support the former assumption.”*

**Lines 421-424:** The sentences

*“On top of the figures we plotted the dates the measurements were conducted (red starts at 24.5 km level). In the case of Lexington the two data gaps higher than 1 month, March and July to September both in 1964 have been left blank in the cross-sections plots. The temporal/vertical cross -section of the aerosols extinctions were generated using a linear time interpolation.”*

Were replaced by:

*“The two data gaps longer than 1 month, March, and July to September both in 1964, have been left blank. The temporal/vertical contours of the aerosol extinctions were generated using a linear time interpolation.”*

**Lines 435-441:** The sentences

*“Regarding the magnitudes of $\alpha_a(532,z)_{US}$ in figure 1, they are slight ly higher than the ones from $\alpha_a(532,z)_{US}$ . That is also the case in figure 3 showing the cross-sections for Fairbanks, with panels similar to figure 2. This is quantified in table 3. At both sites the mean and maximum values for $\Delta\tau_{a*}$ and $\Delta\alpha_{a*}$ are positive showing that the magnitudes of $\alpha_{aUS}$ and $\tau_{aUS}$ are in general higher than $\alpha_{a*}$ and $\tau_{a*}$. Also in the table we appreciate that the magnitudes of the mean percent difference increase of both variables is around 1%. The fact described above disagrees with the possibility **G-66** mentions about lower aerosol backscatter from the retrieval they conducted, using the 1962 US Standard Atmosphere, and the more realistic ones using soundings.”*

Were replaced by:

*“The magnitudes of $\alpha_a(532,z)_{US}$ in are slightly higher than the ones from $\alpha_a(532,z)_*$ for both sites, and it is also true for $\tau_{a_{US}}$ and $\tau_{a*}$. This is quantified in table 2. The magnitudes of the mean percent difference increase of both variables is around 1%. This difference disagrees with G-66 where he found retrievals using the 1962 US Standard Atmosphere slightly lower than the more realistic ones using soundings, but the differences are within calculated errors.”*

**Lines 451-453:** The sentences

*“The values in the denominators $M_{dUS}$ and $M_d$ are the mean values of $N_{dUS}(z)$ and Nd(z) between 25 and 30 km respectively, replicating the procedure used by **G-66**. In figure 4 the differences $\Delta N_d(z)$ for all the 66 soundings at Nantucket used to calculate $N_d(z)$ and the 9 for Fairbanks are plotted. For Lexington, on panel a), $N_{dUS}$ (z) values are both negative and positive, but higher values of $N_{dUS}(z)$ dominate.”*

Were replaced by:

"$M_{dUS}$ *and* $M_d$ *are the mean values of $N_{dUS}(z)$ and $N_d(z)$ between 25 and 30 km, replicating the procedure used by **G-66**. In figure 3 the differences $\Delta N_d(z)$ for 66 soundings at Nantucket and the 9 for Fairbanks are plotted. For Lexington, $\Delta N_d(z)$ values are both negative and positive, but higher values of $N_{dUS}(z)$ dominate.”*

**Lines 463-469:** The sentences

*“Also figure 5 shows the monthly mean $\tau_a$ for the northern hemisphere (Sato et al., 1993). The means for the entire period of measurements available at each site are 0.0215 and 0.0099 respectively. The magnitude of the mean $\tau_{a*}$ at Fairbanks are half that of Lexington, providing evidence of the decreasing aerosol amount with increasing latitude. At the same time, some of the daily $\tau_{a*}$ values at Lexington are around the magnitude of the mean $\tau_{a*}$ at Fairbanks, because of the variability of $\alpha_a(532,z)_*$. Few $\tau_{a*}$ values from Lexington have*

*magnitudes near the values of Sato $\tau_a$, the current reference for this period. However, as we will see in the next section a better agreement is found when the measurements are corrected by two way transmittance attenuation."*

Were replaced by:

*"The means for the entire period of measurements available at each site are 0.0215 and 0.0099 respectively. Also shown is a monthly mean $\tau_a$ for the northern hemisphere (Sato et al., 1993). The mean $\tau_{a*}$ at Fairbanks is half that of Lexington, providing evidence of the decreasing aerosol amount with increasing latitude. Because of the variability of $\alpha_a(532, z)_*$, $\tau_{a*}$ values from Lexignton vary widely from the Fairbanks mean to the Sato magnitude, the current reference for this period. However, as we will see in the next section better agreement is found when the measurements are corrected with two-way transmittance attenuation."*

**Line 456 (Former Figure 2 caption, currently Figure 1):** The sentences

*"The red stars indicate the dates the measurements were conducted. The measurement gaps longer than 1 month, March, and July to September both in 1964, have been left blank."*

Were added at the end of the Figure 1 caption.

**Lines 522-528:** The sentences

*During the course of more than two decades after the pioneering stratospheric aerosols measurements with lidar work by Fiocco and Grams (1964) multiple researchers contributed to the development of the processing algorithms to retrieve aerosols optical properties and its errors (Russell et al, 1979, Klett, 1981; Klett, 1985, Kovalev, 2015). Those facts explain the limitations that do not allow the retrieval of the full set of optical variables characterizing the stratospheric aerosols from the Fiocco and Grams dataset. However using a Junge size -distribution model, and assuming Mie scattering with refractive index 1.5, they produced estimates of the aerosol content of the stratosphere at 16 km: number concentration, surface area and the aerosol density per unit volume of air.*

Were replaced by:

*"Since the pioneering lidar work by Fiocco and Grams (1964) multiple researchers have contributed to the development of the processing algorithms to retrieve aerosol optical properties and errors (Russell et al, 1979, Klett, 1981; Klett, 1985, Kovalev, 2015). These works explain the limitations on retrieving the full set of optical variables characterizing the stratospheric aerosols from the Fiocco and Grams dataset. However assuming a Junge size-distribution model and Mie scattering with refractive index 1.5, Fiocco and Grams did produce estimates of the aerosol content of the stratosphere at 16 km: number concentration, surface area, and the aerosol density per unit volume of air."*

**Lines 539-543:** The sentence

*"An additional validation of those results, in particular for $\tau_a^{Ta}(532, z)$ at Lexington appears in figure 9, where the stratospheric $\tau_a(532, z)$ for the northern hemisphere from January 1964 to July 1965 has been plotted (Sato et al., 1993). The magnitude of $\tau_a^{Ta}(532, z)$ is the same at Lexington (and also at Fairbanks, figure 8) as the $\tau_a(532, z)$ from Sato et al., (1993)."*

Was erased.

**Line 703-704:** The sentence

*"The search for original records should include looking for the at least 25 missing profiles from the total of at least 100 Fiocco mentions".*

Was replaced by:

*"Future search for original records should take into account also the 25 missing files from the more of 100 referred by Fiocco."*

6. Multiple words were replaced to improve and make easy to understand the manuscript. They could be seen in the manuscript with the changes not accepted.

## References


Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, *Geosci. Model Dev.*, **9**, 1937-1958, https://doi.org/10.5194/gmd-9-1937-2016, 2016.

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