ESSDD

Interactive
comment

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.

Anonymous Referee #1

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1   Major Comments

The authors report on the reanalysis of historic first lidar measurements of strato-spheric aerosol in the 1960s. Rescueing such old datasets, and re-evaluating them is a very worthwile undertaking, well suited for ESSD. Unfortunately, I find this is a very wordy and lengthy manuscript, which confuses me with lots of unnecessary or even irrelevant information. Generally the manuscript would benefit much from focusing, substantial shortening (by 30% or more), and also from English editing. I provide a few

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Discussion paper

example lines / sentences below, but nearly all sections could take much more short-ening, e.g. the lengthy introduction about model simulations of stratospheric aerosol, which are motivation, but in detail not relevant here.

The complicated derivation of $\alpha_a(532, z)$ in section 2.4 does not make sense to me. G-66 list $SR_o(694, z)$, but without correction for the 2-way transmission T_{2w}^2 due to Rayleigh-extinction, ozone absorption, and aerosol extinction. The way I read it, the authors then derive $\beta_a(694, z)$ from $SR_o(694, z)$ (their Eq. 6), again without correc-tion for the 2-way transmissions. This step requires atmospheric density profiles, and Rayleigh backscattering cross-sections (their Eqs. 3 to 5). They then correct $\beta_a(694, z)$ for the 2-way transmissions due to molecular Rayleigh-extinction and ozone absorption, but not for the 2 way transmission due to aerosol scattering, and go on to derive $\beta_a(532, z)$ (their Eq. 7, using wavelength dependence from Jäger and Desh-ler, 2002, 2003, both missing in the references, see also Jäger et al., GRL, 2005, https://doi.org/10.1029/95GL01521). Then $\beta_a(532, z)$ is converted to $\alpha_a(532, z)$ (us-ing Jäger and Deshler 2002, 2003), and $\alpha_a(532, z)$ is corrected for two-way aerosol transmission (Eqs. 9 and 10). At the end of this process, $\alpha_a(532, z)$ is not consistent with $\beta_a(532, z)$, because the derived $\alpha_a(532, z)$ was not fed back into the derivation of $\beta_a(532, z)$). Results at 532 nm are also very far from the original measurement, $SR_o(694, z)$, or $\beta_a(694, z)$ at 694 nm.

In my opinion, it would be much more logical (and more accurate) to iteratively de-rive a consistent pair $\beta_a(694, z)$ and $\alpha_a(694, z)$ from the tabulated $SR_o(694, z)$ of G-66. The initial step would neglect aerosol extinction $\alpha_a(694, z) = 0$, and derive $\beta_a(694, z)$ (similar to Eqs. 2 to 6), but with appropriate corrections for the 2-way transmissions due to Rayleigh-extinction and ozone absorption (similar to Eq. 7). It is important, and should be mentioned, that at 694 nm and over the altitude range from 12 to 25 km considered here, these 2-way transmissions corrections are small, each less than 2%, because ozone absorption and Rayleigh extinction are very small at 694 nm in the stratosphere. The combined 2-way Rayleigh and ozone transmission at 694

nm through the stratosphere is about 0.97, very close to 1. In the next iterations, $\alpha_a(694, z) = EB(z) \cdot \beta_a(694, z)$ is assumed, and the calculation steps are repeated, including the estimated 2-way aerosol transmission (from $\alpha_a(694, z)$), and provide a new estimate for $\beta_a(694, z)$. Compared to the combined 2-way Rayleigh and ozone transmission ($\approx$0.97), the 2-way aerosol transmission is smaller ($\exp(-2 \cdot 0.04) \approx 0.92$ according to Fig. 9), so accounting for it is more important. The iterations are repeated, and usually converge after 3 to 5 steps. At the end they provide a consistent pair $\beta_a(694, z)$ and $\alpha_a(694, z)$. Note that this is not the case in the current approach of the authors, becaused the derived $\alpha_a(532, z)$ cannot be fed back into the calculation of $\beta_a(694, z)$. If $\alpha_a(532, z)$ is desired, it can be estimated from $\alpha_a(694, z)$ in a final step (using Jäger and Deshler 2002, 2003).

I think the authors need to explain / justify why their approach is valid, and why the approach suggested above was not taken.

Given this problem, as well as the very lengthy and cumbersome text, I feel that the paper needs major revisions. In doing these, the authors should remember that conciseness is very important for any scientific paper. Most readers will be familiar with the basics, and only need to be informed about important and new results. There is no need to start from Adam and Eve, which tends to be the case here.

2 More Specific Comments

Abstract: Somewhere, it should be mentioned that the primary quantity measured by a stratospheric lidar (and also produced in the dataset) is the backscatter ratio or the aerosol backscattering coefficient, not the extinction coefficient. Extinction is a derived / secondary quantity. It relies much more on assumptions (about the extinction to backscatter rato, also called lidar ratio) than backscatter. Extinction is usually small, but is necessary to derive the best possible backscatter profile.

line 56: something missing / incomplete sentence?

line 77/78: something missing / incomplete sentence?

line 106/107: replace "very high frequency" by "short pulse". The text says "nano-second", but Table 1 says $< 1\mu$sec. What is correct? I would assume micro-second, because in the 1960s nano-seconds were very hard to achieve, and even harder to measure.

line 107 to 110: Both sentences could easily be deleted.

line 111: after "detected", add "between 10 and 30 km altitude"

line 150: add "in the stratosphere" after T_{2w}^2

line 155: Add "This is a good assumption for times of low stratospheric aerosol loading. For enhanced stratospheric aerosol, e.g. after volcanic eruptions, however, aerosol extinction becomes important, reduces the stratospheric transmission, and makes it range dependent."

lines 159 to 161: This could be said much shorter and better. Just say "In a final step, each profile was normalized to one between 25 and 30 km."

lines 162 to 166: Again, very wordy and lengthy. Should be shortened.

lines 180 to 230: lengthy and very confusing!! People unfamiliar with stratospheric aerosol lidar will be totally confused. People familiar don't need this part, but will be puzzled now. What was done? From G-66 and Eq. 1 you can get the lidar return signal $\frac{dn(z)}{dt}$. From that you can go through the process.

lines 212 to 234: I don't understand this discussion, and I don't think it is necessary. Total aerosol optical depth is not needed, only stratospheric aerosol depth is needed between 12 and 25 km, the altitudes tabulated in G-66. The authors do not need two-way transmission from the ground to z, they only need stratospheric two-way transmissions from the normalization altitude (or 25 km) down to z. Since total aerosol optical

depth is very variable, and usually dominated by the troposphere, any use of total optical depth data here is frought with large uncertainties. Lines 290 to 340 and Fig. 1 are also not needed for the same reasons. I strongly suggest to remove all this confusing and unnecessary material.

Sections 2.5, 2.5.1, 2.5.2, very long and wordy. should be shortened substantially. The key points should have already been said / explained in the description of the method to get from $SR_o(694, z)$ tabulated in G-66 to $\beta_a(z)$ and $\alpha_a(z)$.

In section 2.5.1: I think the authors need to explain, here or when they decribe their algorithm, that the US-Standard Atmosphere density profile needs to be backed out from $SR_o(694, z)$ tabulated in G-66 to get the original lidar return signal $\frac{dn(z)}{dt} \cdot \frac{z^2}{K}$ in Eq. 1, and that then better / newer profiles are used to derive $\beta_a(z)$.

Also in 2.5.1 and 2.5.2, it needs to be stated that only the stratospheric parts of the two way transmissions due to Rayleigh-extinction and ozone absorption are needed, and that both of these are almost constant at ≈ 0.98, and very close to 1. This means that it is essentially irrelevant, which atmospheric profiles are used to account for the two transmissions.

Section 2.5.3: Should be removed entirely, as mentioned above.

Section 2.7.2: I think it would be very helpful to see a typical altitude profile of the overall error, and the different contributions. I would expect, that the contributions from Rayleigh and ozone two-way transmissions are quite small, and that other terms dominate.

Section 3.1: should $\beta_m(594, z)$ not be $\beta_m(694, z)$ throughout this section (and in a few other places)? To me, it would be clearer in most places to say "density profile from the US Standard Atmosphere" and "density profiles from local radiosondes" instead of "$\beta_m(694, z)$ from the US Standard Atmosphere" and "$\beta_m(694, z)$ from the soundings"

Figure 4: it would be good to have two more panels showing the difference (or the ratio)

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of $\beta_{a,US}(694, z)$ and $\beta_{a,Radiosondes}(694, z)$ (or $\alpha_{a,US}(694, z)$ and $\alpha_{a,Radiosondes}(694, z)$). Fig. 2 and 3 could even be dropped then.

Figures 5, 8, 9: the indicator symbols at the top axes are not needed and confusing. They should be removed.

Figure 9 contains the same information as Fig. 5, and is more comprehenisve. Fig. 5 should be dropped. The entire discussion of results without correction for aerosol 2-way transmission should be shortened substantially. We know these results are poorer, and Fig. 9 shows it very clearly. There is not point in lengthy discussions of things that are obsolete or have been superseded.

After Figs. 6 and 7: It might be good to have a Figure similar to what I suggest above for Fig. 4, but showing the differences between profiles with and without correction for aerosol 2-way transmission.

Figure 8: these results should be included in Fig. 9, and Fig. 8 dropped.

Lines 581 to 586: This is a very complicated way of saying that the errors in the aerosol parameters blow up, when there is little aerosol, and SR is close to 1.

Lines 587 to 592: A very complicated way of saying that uncertainty due to aerosol extinction correction becomes large, when aerosol extinction becomes large.

Lines 597 to 618: Difficult to read, and difficult to find take-home messages.

Section 3.4: Given the overall length of this data paper, and to maintain a better focus, I would suggest to drop this entire section, including Figs. 12 and 13. This is supposed to by an ESSD data paper, not an ACP paper.