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Discussions

Interactive comment on "Variability and evolution of mid-latitude stratospheric aerosol budget from 22 years of ground-based lidar and satellite observations" *by* Sergey M. Khaykin et al.

Anonymous Referee #3

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Lidar aerosol observations from Observatoire de Haute-Provence (OHP) in Southern France have been made since the 1980s, but have not been summarized in the literature since the early 1990s, which is the purpose of this paper. After reviewing the basis of the record and the conversion to a common 532 nm wavelength, the authors convert lidar backscatter measurements to extinction to calculate stratospheric aerosol optical depth (sAOD) from 17-30 km for comparison with a number of satellite aerosol instruments. The agreement is remarkably good and this result will be a nice addition to the literature.

The authors then use the global CALIOP data base to begin making sweeping conclusions about the processes controlling the stratospheric aerosol observed at OHP.

While the measurements presented are at times consistent with the statements about processes, the statements made about processes are not definitively established with the analysis shown. Thus the conclusions extend beyond what is established in this paper and major sections of the paper should be reconsidered and carefully rewritten to reflect that the processes described are not definitive and may be tempered by many complicating factors.

When the authors stay close to their data and make a definitive separation between measurements classified as volcanic and non-volcanic, and present data representative of these two states, then useful results are obtained. This should be more carefully presented and discussed, and the discussions of global processes treated less definitively. Here are some details.

265-267. The authors state, "Both SAGE II and OHP lidars report an average background sAOD1730 for the "reference" quiescent period of 2.3Âů10-3 $\pm$ 2.4% (2 SE), which is marked in Fig. 3 by dashed line and grey shading, indicating 1-σ range of values." This statement implies that sAOD should be between 0.00225 and 0.00235 since 2.4% of 0.0023 is 0.00005, yet the range shown in the figure is much larger than this. The authors claim that 2.4% is 2 SE, which, the reader is left to assume, means 2 standard errors. Then the authors say the shading represents 1-σ, without explanation. So in the end the reader is unsure what is shown in the figure, but it seems to be larger than 2.4% of the mean value quoted and how does 1-σ compare with 2 SE?

231-234. It is very difficult for the reader to understand how the figure supports these statements. Above 25 km the lidar data do not show any particularly different bias compared to satellite than below in the left panel of Fig. 2. The lidar data lie within the symbols for both SAGE and OSIRIS. On the right panel the lidar data split the satellite data and the agreement is overall better than below 25 km. Below 25 km the agreement with CALIOP remains good but is worse OSIRIS and OMPS.

288-301. Surely the differences between the plumes of Sarychev and Nabro are primarily driven by the significantly different latitudes of the two eruptions, compared to the latitude of OHP, and the dominance of the mixing by zonal flow in the stratosphere. Sarychev, at nearly the same latitude as OHP, is detected very early and the volcanic plume appears as pulses of aerosol, as these pulses are advected around the Earth before they are significantly mixed by the general flow. In contrast the aerosol from Nabro is already well mixed by the general flow prior to its arrival at OHP, 45 days after the eruption. To effectively compare the evolution of these two eruptions the color scales should be adjusted to both start at the day of first detection of Nabro, 45 days after each eruption. All profiles prior to this time from Sarychev could be indicated as black profiles.

319-320. “The plumes of more distant (tropical) eruptions are not always obvious in sAOD series.” What is a more distant tropical eruption? Nabro is tropical. Considering the dominant zonal flow does the longitude of a tropical eruption make a big difference? Why are these “more distant tropical eruptions” not evident in sAOD series? Is this sAOD now meant to only imply sAOD at OHP? Distant and close tropical eruptions will make a difference in sAOD depending on where sAOD is measured, but the reader is left to guess what is intended. The text implies that the plume from a volcanic eruption has a rather direct stratospheric transport to the mid latitudes from a tropical eruption, but doesn’t the dominant zonal flow in the extra tropical stratosphere confound this idea?

336-337 and Fig. 5. “Aged” is not a very descriptive term. Better would be some consistency such that the volcanic curves represent an average of the measurements over some specified time period, which ideally would be the same time after each eruption.

353-357. The CALIOP data are far from clearly supporting the suggestion that the plume from Merapi was observed at OHP. The structure in the CALIOP data at OHP latitude in early 2011 which coincides with the blue shading in the OHP data has an origin prior to Merapi, whereas it is not obvious that the plume from Merapi is still intact at

45°N. The sAOD1519 from CALIOP is 2e-3 to 3e-3 compared to 5e-3 at OHP. In contrast after Nabro in mid to late 2011 the CALIOP data display a significant increase in aerosol at OHP latitudes whereas OHP sAOD is hardly larger than the value attributed to Merapi. Such discrepancies raise questions about how well these two data sets really agree, particularly at these altitudes. Is this reflective of the differences between the OHP and CALIOP measurements below 16 km in Fig. 2b. This seems unlikely. Figure 6, in the discrepancies of the timing between OHP sAOD and CALIOP sAOD for both Nabro and Sarychev, raises question about the correspondence of these two data sets. At the very least the timing of Sarychev, Nabro, and many of the aerosol minima appear to be displaced, with OHP lidars lagging the CALIOP data.

367-372. Fig. 6 displays 10 years of CALIOP AOD from 15-19 km from 60S to 60 N. What fraction of the troposphere is included here? Certainly in the equatorial and tropical regions there is about 1-2 km of tropospheric data since the tropopause is typically near 17 km. The upper troposphere can be quite clean if there is deep convection or it can be influenced by tropospheric aerosol. To attribute all the data shown in Fig. 6 to the stratosphere is misleading. Here the authors want to suggest based on signatures, clouded by the uncertainties just mentioned, that 4 of these 10 years display evidence of the ATAL. But how would the ATAL be separated from other aerosol laden air from the upper troposphere? What other evidence is there to link this slight change in AOD to the ATAL? Is it really so clear in terms of the timing of these events? How similar is it? Finally this is a paper about the OHP lidar record not a broad scale interpretation of the CALIOP data from 60 S to 60 N. If the latter is the intent then do a complete job on the CALIOP observations. Here the intent appears to be on the OHP lidars. If so then there should be a better discussion of when the CALIOP is in agreement with OHP, when it is not, and why there are differences.

373-382. This picture is a bit less clear than suggested. Many of the Northern Hemisphere low aerosol tongues are rather discontinuous even when volcanoes are not involved. The lidar and CALIOP timing of the low aerosol load are different. While there is some evidence for the author's assertion, it is far from definitive, and other processes may be involved. The influence of the troposphere on the AOD displayed is unclear. It is also not clear to what extent a higher summer tropopause would affect the OHP data compared to a lower tropopause in the winter. If the authors wish to pursue this type of interpretation of the CALIOP data they should consider preparing a paper focused on such analysis of the CALIOP data and not add it as a sidelight to this paper about OHP lidars.

Fig. 7a and 7b display several discrepancies. CALIOP data display the expected Junge layer with minimums below 18 and above 24 km, and a maximum near 20 km throughout the year. OHP suggests a significant modulation of the Junge layer with a decrease of AOD from 1.08 to 1.04 from April to December which is not seen in the CALIOP data. Is this seen in other data sets? It is not clear what would cause this modulation of the Junge layer. The CALIOP data do not show a strong increase in aerosol near 16 km in the autumn. The authors explain this away as due to zonal averaging. But really is the connection so immediate, from the Asian monsoon to 45°N, that the ATAL would only appear in the OHP data? Is the ATAL signal so small that it is diluted with the zonal average, even though that average would incorporate much more of the Asian monsoon outflow than would reach OHP?

Why are the time periods covered by Fig. 7a, 7b so different? Is there a point to be made about similarities of any non-volcanic period, or is the point to show how similar the OHP lidars are to CALIOP? If the latter then wouldn't it be better to compare the same time frames?

525-526. Calling the authors' explanations for the observations "rather robust" is not justified in this reviewer's mind, and suggesting there may be alternate explanations, which are not explored, but should be, is less than genuine at this point in the conclusions.

The discussion section is a recap of the conclusions reached based on the analysis

discussed above which I find incomplete and perhaps misleading. The models the authors have to characterize the data are too simplistic and ignore many complicating factors.

Minor comments:

870. embedded panel? Do the authors mean the legend?

175. I am not quite sure what is meant by occultations for a limb scatter instrument. What is being occluded?

291. 3.4 units? Do the authors mean a scattering ratio of 3.4?

307-308. Why do the satellite measurements not agree with the optical depth decrease after January 2015 observed by the OHP lidars? Rather the satellites remain elevated at the January level.

309. This comment on Calbuco is not really necessary here since it does not affect post Nabro OHP and forces the reader to look ahead to Fig. 6 to verify the statement, which is then called out of order.

323. What is the partial sAOD examined? Is it the same for all satellites? It should be stated what the AOD covers.

324. Another call out to Fig. 6 out of order. Should the figure orders be reversed?

329-332. "monthly-mean sAOD1730 and SR" where? Is this for OHP only or does it include all the satellite data? In ii) specify the "reference" quiescent period, e.g. 1997-2003. 336. Concerning the quiescent period, the text and Fig. 5 caption state 1997-2003, the legend in the figure states 1998-2003? These should be consistent. 365-366. "The enhanced poleward transport into the winter hemisphere is exhibited by meridional wind vectors in Fig. 6." Then according to the figure there is no meridional wind after 2009. Is this correct?

Fig. 7 caption. The reader does not know what is meant by "SR from OHP LiO3S lidar for selected volcanically-quiescent periods . . .” What is the selection based on? Is it all non-volcanic periods or just select periods?

428-429.”Importantly, for any quiescent subperiod over the course of 22 yr OHP series, the pattern is essentially the same.” The OHP lidar record is only 22 years long, so what does this statement mean? Do the authors mean any quiescent subperiod within the 22 year data record?

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