

Reply to Referee#3

We thank Anonymous Referee #3 for a focused attention to our study and a detailed review.

Before reading this reply please refer to the Authors' comment published in the interactive discussion: AC1 "Reprocessing of the OHP lidar data and related changes to the manuscript".

The revision applied to the data and the updated discussion thereof are expected to help reducing the skepticism expressed by the Referee #3. However, in the reply that follows we tried to thoroughly address each remark, providing occasionally figures that support our interpretation. We also emphasize that our interpretation of mid-latitude aerosol variability relies on the existing and widely accepted studies.

265-267. The authors state, "Both SAGE II and OHP lidars report an average background sAOD₁₇₃₀ for the "reference" quiescent period of $2.3 \cdot 10^{-3} \pm 2.4\%$ (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?

The sentence was supposed to mean that the average background sAOD₁₇₃₀ is $2.3 \cdot 10^{-3} \pm 2.4\%$ (2 SE), where 2.4% is two times relative standard error obtained as standard deviation of monthly-mean values of sAOD₁₇₃₀ during the background period divided by a square root of the number of these values and expressed in percentage from the average background sAOD₁₇₃₀ ($2.3 \cdot 10^{-3}$). The standard deviation itself amount to 12.6% (OHP lidars) and 10.2% (SAGE II). To avoid confusion the mention of standard error was replaced by standard deviation and the sentence was modified as follows: "According to the mean of OHP lidars, the average background sAOD₁₇₃₀ for the "reference" quiescent period of $2.37 \cdot 10^{-3} \pm 12.6\%$ (1 σ), which is marked in Fig. 3 by dashed line and grey shading, indicating $\pm 1\text{-}\sigma$ range of values. SAGE II reports sAOD₁₇₃₀ for the same period of $2.4 \cdot 10^{-3} \pm 10.2\%$."

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.

After the reprocessing of OHP data (See AC1 "Reprocessing of the OHP lidar data and related changes to the manuscript"), Fig. 2 and associated description have been fully revised:

"Figure 2 displays a comparison of aerosol extinction profiles averaged over two 20-month volcanically-quiescent periods 2002-2003 and 2013-2014 covered by time-overlapping observations by two different triplets of satellite sounders. The comparison reveals close agreement between OHP lidar, SAGE II, GOMOS and OSIRIS (Fig. 2a) above 15 km and somewhat poorer agreement below. Fig. 2b suggests a good agreement between OHP lidar and CALIOP (relative difference 5-10%) throughout the entire range of altitudes except the uppermost layer above 25 km, where OHP lidar is 15-20 % low with respect to CALIOP. This feature may be related to an error in lidar calibration, relying on the assumption of the absence of aerosol above 30 km, which – as suggested by CALIOP data calibrated at higher altitudes – may not always be the case. The other two satellite sounders covering 2013-2014 period – OSIRIS and OMPS - show somewhat larger discrepancies (reaching 30%) with OHP lidar and

CALIOP in the uppermost and lowermost layers. This discrepancy may be due to the use of a fixed lidar ratio and wavelength exponents, which may vary with height depending on the size distribution of aerosol.”

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.

After a careful profile-by-profile data screening (see AC1), the considerations on the detection timing of Sarychev and Nabro plumes have been entirely revised. As a matter of fact, the delay between eruption and plume detection at OHP is the same for Sarychev and Nabro eruptions. The first unambiguous detection of Sarychev plume (erupted 12.06.2009) dated 26.06.2009, whereas Nabro plume (erupted 12.06.2011) was detected on 28.06.2011, that is 15 days after the eruption (cf. 45 days reported previously). Figure 4 was updated with the profiles previously discarded and the colour scale was uniformed for both Fig.4a and 4b. The respective description in Sect. 4.2.1 (former Sect. 4) has been revised as follows:

“A better insight into the temporal evolution and vertical structure of Sarychev and Nabro plumes is provided by Fig. 4, showing scattering ratio (SR) profiles obtained by OHP LiO3S lidar during the corresponding volcanic periods and converted to 532 nm. The plume of Sarychev was detected at OHP 14 days after the eruption as sharp SR enhancements in the lowermost stratosphere reaching a maximum value of 4.8 at 15 km (30.06.2009). On 15.07.2009 a sharp enhancement in SR of 2 was observed by LiO3S as high as 21.7 km. The presence of aerosol at this level is confirmed by LTA observations on the next night (not shown), which reported SR at this level reaching a value of 3.5. A remarkable scatter between the individual profiles points to a rapid three-dimensional evolution of the plume (Jegou et al., 2013), dispersed by the stratospheric mean zonal flow, which reversed over the course of the plume permanence.

The first signatures of Nabro plume were detected at OHP already 15 days after the eruption: a strong peak in SR reaching 2.8 was observed at 16.5 km on 28.06.2011 (Sawamura et al., 2012). Over the course of July, several relatively thin (<1 km) aerosol layers with SR below 1.6 were detected between 14 and 17 km altitude. Starting from early August (~50-60 days after eruption) the plume of Nabro – as observed at OHP – expands in altitude and obtains a smoother shape indicating the arrival of air masses, in which the aerosol-laden air is mixed with the ambient air by the general flow. Broad (~3 km) enhancements in SR of ~1.5 centered at 17 km were observed at OHP through March 2012.”

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?

Technically, among all the VEI=4 eruptions since 1994 Nabro is closest to OHP in absolute distance. Also, as can be inferred from the analysis of dispersion of Nabro plume (e.g. Sawamura et al., 2012; Fairlie et al., 2014) the longitude of eruption may make a large difference, especially in the context of detection of volcanic aerosol at a mid-latitude site. Eventually, the dominant zonal flow would uniformly distribute the aerosol load longitudinally, however the efficiency of meridional transport of a volcanic plume depends strongly on the season and location of the eruption. A rapid transport of Nabro plume to Mediterranean region was ensured by the circulation around Asian monsoon. In contrast to that, aerosol from eruptions occurring elsewhere within the tropical belt would tend to remain mainly in the tropics, while their transport to Northern mid-latitudes would be inhibited during Boreal summer, when the subtropical mixing barrier is stronger. For this reason there may be a substantial delay between the eruption and the time when the aerosol-laden air has reached mid-latitudes. By that time, the air is already very well mixed with the environment and hence the associated enhancement in aerosol loading as observed at OHP appears “less obvious”. This is what we refer to as an “aged” plume.

The respective sentence was modified for clarity: “The plumes of more distant (tropical) eruptions are not always obvious in ~~sAOD-series~~ OHP observations”.

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.

The term “aged” describes the time lag between an eruption and its detection at the measurement site. As discussed in response to the previous remark, the timing and location of an eruption plays a crucial role in how soon the volcanic aerosol is transported to OHP latitude. The altitude of volcanic injection is also important in this context. Aerosol from high-altitude injections (e.g. Soufriere Hills, Kelud), would remain in the stratosphere for a considerable period, however, the timescale of poleward transport of their plumes (mainly through Brewer-Dobson circulation) may be long. In contrast to that, the aerosol from volcanic injections into the TTL (e.g. Tavurvur) will be removed more rapidly (through mixing and/or cloud scavenging), but its transport to mid-latitudes may be faster thanks to more efficient meridional exchange at those levels. Thus, each eruption requires an individual approach, which makes use of both global and local observations.

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.

Indeed, it is not obvious that Merapi plume has actually reached the OHP latitude. CALIOP data show the northern boundary of the plume at around 40° N. OSIRIS data suggest that Merapi plume did not extend beyond 35° N. The reason why Merapi plume was said to be observed at OHP was that all criteria for selection of volcanically perturbed periods were fulfilled for this eruption, namely a) propagation of plume to the Northern mid-latitudes (as inferred from CALIOP) and b) criteria i) and ii) regarding sAOD and SR (Sect. 4.4, former Sect.

4.1). However, after reprocessing of OHP data (see AC1) the criteria applied to the lidar data are no longer fulfilled. Indeed, the reprocessed lidar series do not provide indication for the increase of aerosol load around the turn of 2011 and this period is no longer considered as volcanically-perturbed.

As far as the consistency between OHP and CALIOP series is concerned, the $sAOD_{1519}$ series from OHP lidar and CALIOP are in fact in good agreement. The upper panel of Fig. AR3.0 shows the CALIOP curve, revealing itself in good correlation with OHP series. We note that during the periods of high aerosol load (posterior to Ok/Ka, Sa and Na eruptions) CALIOP shows smaller AOD_{1519} values compared to OHP lidar. In CALIOP retrieval the attenuation due to aerosol is not corrected for. However, with the two-way transmission near 0.97-0.98 (for AOD 0.010-0.015) the error on the AOD at 15 km would not be bigger than 2-3%.

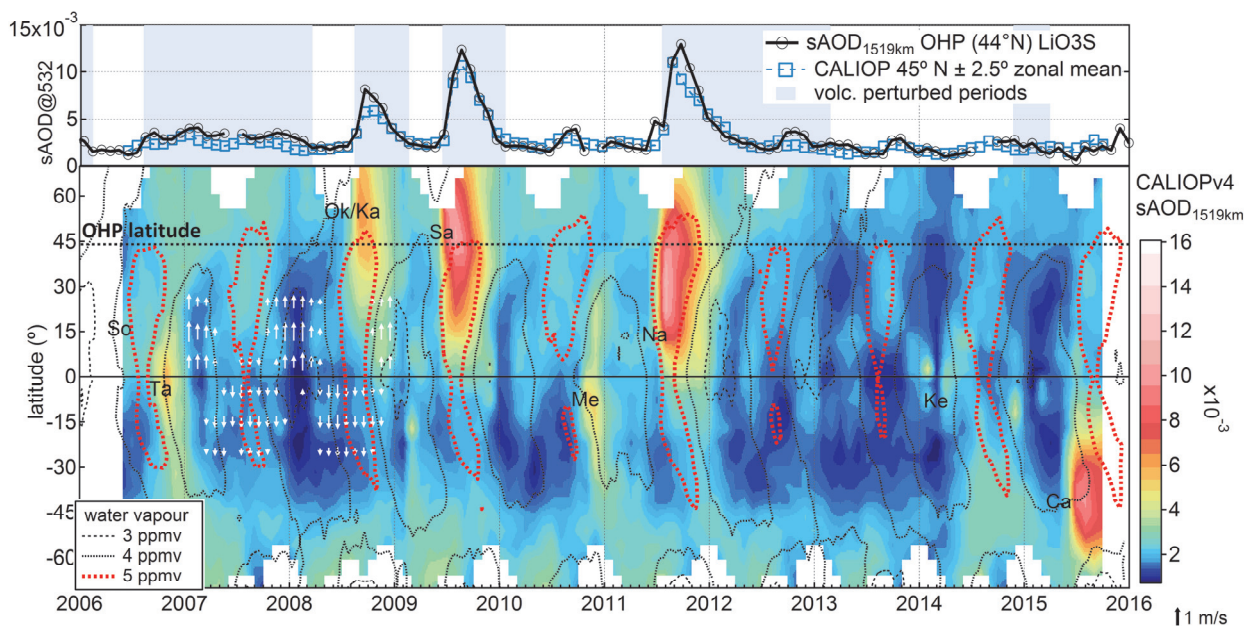


Figure AR3.0. Time series of monthly-mean $sAOD_{1519}$ from OHP LiO3S lidar and CALIOP (top) and time-latitude section of $sAOD_{1519}$ from CALIOP in log-scaled color map with indications of VEI 4 eruptions (bottom). Time periods considered as perturbed by volcanism (Tab. 2) are shaded light blue in the top panel. White arrows (in 2007-2008) represent the mean meridional component of monthly/zonally-averaged horizontal wind at 100 hPa from ERA-Interim reanalysis. Dashed and dotted contours depict zonal-mean water vapour mixing ratio at 100 hPa from Aura MLS.

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.

We do not mean to attribute all the data in Fig. 6 to the stratosphere. In the tropics, 15-19 km layer includes a part of the TTL, although mostly above LZRH – Level of Zero Radiative Heating – above which the air tends to rise (Fueglistaler et al., 2009). Meanwhile, at OHP latitude it is entirely in the lower stratosphere. The figure is intended to show the processes that affect the variability of stratospheric aerosol at mid-latitudes, whatever layer of the atmosphere is at play. Notation “sAOD1519” for CALIOP in Fig. 6 is chosen for compatibility with that for OHP. For the sake of better precision it was changed to AOD1519 for CALIOP.

ATAL signatures in CALIOP data in Figure 6 **alone** would not allow us to unambiguously attribute the slight positive anomalies in aerosol to ATAL. However, if considered together with the available knowledge of ATAL’s three-dimensional extent and seasonality available from the literature (e.g. Vernier et al, 2015) and commonly accepted, this feature can hardly be attributed to anything else but the phenomenon of ATAL. As an additional support to our statement Fig. AR3.1 shows CALIOP AOD for the layer 15-16.5 km, where ATAL occurrence is more prominent.

We do not intend to provide a broad scale interpretation of CALIOP data from 60S to 60N; this can be found elsewhere in the literature quoted throughout our paper. The rationale behind showing the global distribution of AOD is threefold: i) to show that background aerosol at mid-latitude is modulated by the global circulation, particularly poleward transport; ii) to identify the processes responsible for the annual cycle of aerosol at mid-latitudes, laying the ground for interpretation of Fig. 7; iii) to point out the similarity in aerosol and water vapour time-latitude patterns, suggesting the same driver for the both.

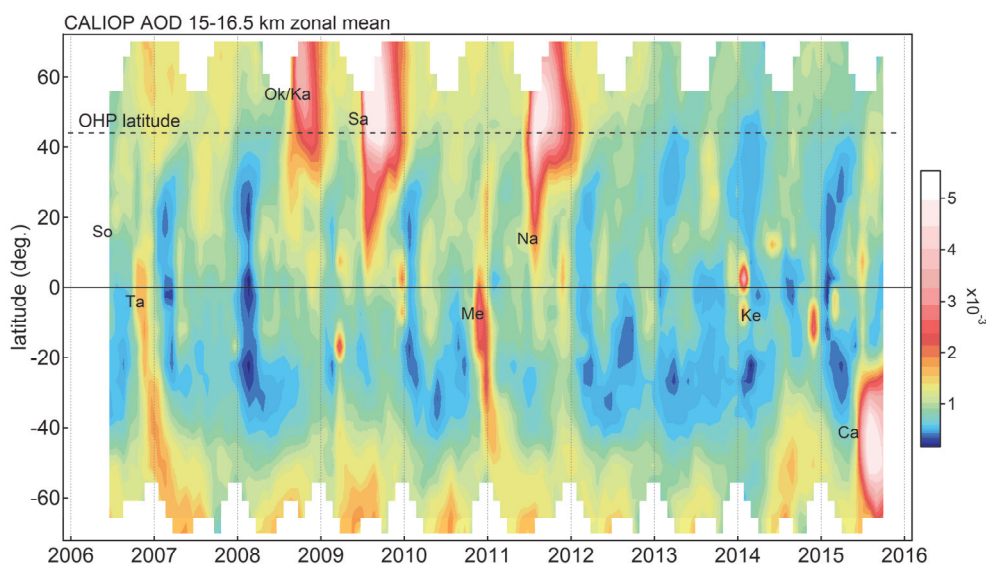


Figure AR3.1. Time-latitude section of AOD in 15-16.5 km layer from CALIOP in log-scaled color map with indications of VEI 4 eruptions. Systematic increase of AOD in the Northern sub-tropics/mid-latitude is attributed to ATAL.

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.

Measurements of very low aerosol concentration are prone to large error, even when zonal/monthly means of CALIOP are used. This is why the tongues may sometimes appear

discontinuous. In order to support the interpretation of clean air tongues, we overplot the water vapour pattern, which emphasises the poleward transport of dry (clean) air, whose composition is set during austral summer. There is no discovery here, we rely on the previous studies and make sure to properly refer to them.

We do not mean to provide a breaking explanation of the aerosol minimum during Austral summer in the tropics, this is already done by Vernier et al., (2011) and widely accepted (Kremser et al., 2016). Alternative processes that might potentially responsible for the LMS aerosol minimum at mid-latitudes are discussed in Sect. 6.

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?

After the reprocessing of OHP lidar data (see AC1) it was possible to extend the analysis of aerosol profiles down to the tropopause. Fig. 7 has undergone a major revision, namely: i) altitude range extended down to 13.5 km in all panels; ii) color scale range in Fig. 7a,b reduced to SR=1.1 for emphasising the background aerosol pattern; iii) CALIOP data in Fig. 7b restricted to $45^{\circ} \pm 2.5^{\circ}$ N (see Fig. AR3.2). In addition, slight change to the aerosol pattern in Fig. 7a,b is due to revision of the volcanic mask (removal of Merapi, see above). In the updated Fig. 7a,b the ATAL signature is better pronounced both in OHP and CALIOP data. It is noted that the onset of ATAL layer occurs earlier in the CALIOP section, which might just be due to zonal averaging, which includes the north-east part of ATAL.

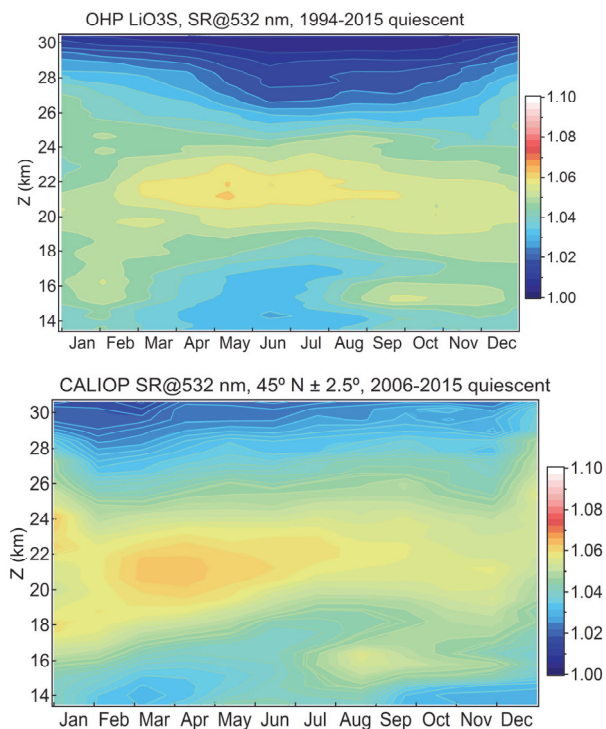


Figure AR3.2. Climatological month-altitude sections of a) SR from OHP LiO3S lidar for volcanically-quietest periods over the entire measurement time span (1994-2015); b) zonal-mean SR at 45° N $\pm 2.5^{\circ}$ from CALIOP, June 2006 - September 2015 for volcanically-quietest periods (Tab. 2)

The discussion around Fig. 7a,b has been revised as follows:

“Fig 7b provides a satellite zonal-mean view on the non-volcanic aerosol annual cycle observed by CALIOP since 2006. The month-altitude pattern of zonal-mean background aerosol revealed by CALIOP supports the climatology observed by OHP lidar. The main features, namely the winter maximum of the Junge layer upper boundary, the spring maximum of SR in the middle layer (19-25 km) and the upward propagation of the late-spring clean feature are readily discernible in both OHP and CALIOP climatologies. The signature of ATAL at 15-16 km altitude is also well pronounced in CALIOP section, which shows its maximum development in August as opposed to September according to OHP climatology. This may be due to zonal averaging for CALIOP, which incorporates the mid-Asian part of Asian monsoon, where ATAL is better developed in August (Fig. 2 in Vernier et al., 2015).

OHP lidar and CALIOP capture well and agree on the main features of background aerosol annual cycle in the lower mid-stratosphere, whereas above 25 km CALIOP shows higher SR values compared to OHP lidar and somewhat less pronounced annual cycle. This may be due to higher altitude of calibration for CALIOP retrieval and the use of different atmospheric models for deriving molecular backscatter (Sect. 2.3 and 3). “

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?

We use the full time span of OHP data for constructing the annual cycle of background aerosol in Fig.7a to enhance the sampling and to reduce the noise. The pattern remains essentially the same if we consider the same time periods for OHP and CALIOP (see Fig. AR3.3). There is a respective mention in Sect. 5.1, end of paragraph 3: “Importantly, for any quiescent subperiod over the course of 22 yr OHP series, the pattern is essentially the same.” The more important point of comparison between OHP and CALIOP annual patterns is to show how similar they are.

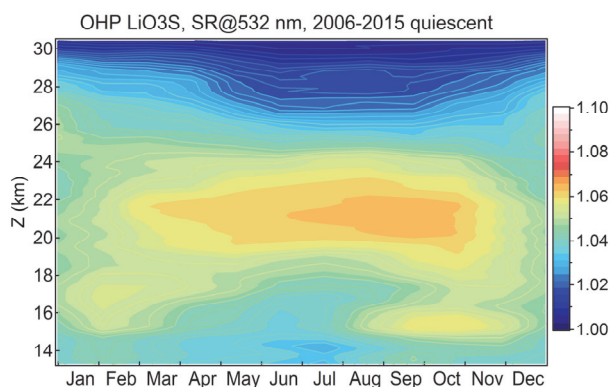


Figure AR3.3. Climatological month-altitude sections of SR from OHP LiO3S lidar for volcanically-quiescent periods over the CALIOP observations period (2006-2015).

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 phrase “rather robust” refers to the main point that is made in the paragraph regarding the control of mid-latitude background aerosol by convective processes followed by poleward transport of clean and polluted air, which represents the main driver of aerosol annual cycle at OHP latitude. We have provided a sufficient amount of observational evidence to this finding using both global and local measurements after having demonstrated the consistency between the both. When referring to the convective processes responsible for cross-tropopause transport of clean or polluted air, we rely on the existing and widely accepted studies. Further, we do consider and discuss the alternative contributors to the observed aerosol annual cycle in the two paragraphs that follow.

The last sentence in paragraph 3, Sect. 6 has been modified: “Although this interpretation appears self-consistent, alternative contributors should also be considered.”

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.

Section 6 (Discussion and summary) complies with its purpose: to discuss alternative interpretations and to provide a recap of conclusions.

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

There is in fact no embedded panel in Fig. 1. The mention of it was removed from the figure caption.

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

The word “occultations” was replaced by “vertical profiles”.

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

Yes, we meant “scattering ratio of 3.4”. The text was corrected accordingly.

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.

A minor and transient decrease of optical depth after January 2015 seen by OHP lidars (Fig.3) is also resolved by the satellite mean series but appears somewhat less pronounced. Note that this decrease is also less pronounced in the revised OHP series.

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.

The mention of Calbuco eruption has been removed from the paper.

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

The sentence has been modified: “The plumes were detected by examining time-latitude sections of sAOD₁₇₃₀ and AOD₁₅₁₉ from all satellite records..”

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

The introduction of Fig. 6 in this section is indeed premature, however it provides a great aid in understanding how the volcanic plumes are detected using satellite data. The sentence has been nevertheless modified : “...(example for CALIOP is provided hereinafter in Sect. 5). The figure order can not be reversed as this would strongly disrupt the flow of presentation.

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.

This is for OHP only. The preceding sentence has been modified: “In this way, a period is considered as volcanically-perturbed if both of the following two conditions are fulfilled in OHP observations:..”. Reference period has been specified in ii): “ii) monthly-mean SR profile exceeds the 1- σ range of the “background” SR profile - an average over the entire “reference” quiescent period of 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.

The legend in Fig. 5 has been corrected to 1997-2003.

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?

No, this is not correct. The figure caption says: "White arrows (**in 2007-2008**) represent the mean meridional component of...". The absence of wind vectors after 2009 does not mean the absence of meridional wind in the atmosphere. The plotting of wind vectors was period-limited to avoid overloading the figure.

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?

By "selected" we meant that the periods were selected on the base of criteria described in Sect. 4.4 (former Sect. 4.1). The word "selected" was removed from Sect. 5.1 and Fig. 7 caption.

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?

Yes, we mean "any quiescent subperiod within the 22 year data record". The sentence has been modified: "Importantly, for any quiescent subperiod within the 22 yr OHP record, the pattern is essentially the same."