

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 #1

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General comments

This paper is based on 22 years of continuous observations of stratospheric aerosols from two independent ground-based lidar systems located at OHP. This very valuable data set is used in synergy with satellite observations from several instruments. After proper scaling, stratospheric aerosols optical depths between 17 and 30 km retrieved from the OHP lidars and from the satellite instruments are in remarkable agreement. The synergetic analysis of OHP and satellite data allows identifying two “quiescent” time periods during which stratospheric aerosols are “background aerosols” with no contribution from volcanic eruptions. The paper ends with the analysis of the variability (annual cycle) and long term change of the background aerosols. The content of this

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manuscript is good, but minor revisions are recommended before publication. The discussion is sometimes difficult to follow and a little too dense, unless the reader is very familiar with the topic. In my opinion, explanations and details are sometimes missing, and equations could be added in some places for more clarity.

Specific comments

Abstract: line 32: define post-Pinatubo era or recall date of eruption

Section 1 Lines 76-80: what are “minor” eruptions in this context? How do they compare with the “strong eruptions” (line 63) of $VEI \geq 4$?

Section 2.1 : OHP lidar instrument:

Lines 113-116: why mention the separate telescope and detection channel if not used for this study? Note that the Hoareau et al. (2013) reference is missing.

Lines 119-121: “the primary low gain channel of LTA.0.03 m² telescope”: are you here describing the “primary, more powerful channel detection channel of LTA” used in this study? The collecting area seems very small. Please clarify.

It is stated that the study is restricted to altitudes above 12-13 km due to saturation issues (lines 115-116). It seems inconsistent with lines 242-244 where it is written that LTA may be affected by incomplete desaturation below 17 km. Please explain the saturation and desaturation issues. Photon counting? Other?

Section 2.2: should the title be “OHP lidars aerosol retrieval”?

Lines 130-133: Please clarify the use of K_b and/or K_e for OHP LiO_3S retrievals. Equations would be very helpful, and would clarify the presentation and discussions in Section 2.3 and Section 3.

Lines 141-148: An important source of uncertainty is indeed the actual aerosol loading in the 30-33 km reference region. This is mentioned later (lines 233-234), but could be discussed in more details here. Could CALIOP V4.00 measurements at 30-33 km be

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used to estimate OHP calibration biases?

Lines 149-156: this discussion is important and could/should be developed. For instance, the sensitivity to the assumed lidar ratio (line 154) should depend on the optical depth?

Section 2.3 Satellite aerosol sounders

It could be interesting to comment on the fact that some instruments measure a backscatter further converted to extinction whereas other measure directly an extinction. What is the rationale for the selection of the various values of K_e ? No conversion to 532 nm is mentioned for GOMOS retrievals. Are they directly at 532 nm? Adding a table summarizing all the conversions applied to the various data sets could be useful.

Lines 184-186: I think that it is important to explain in the text that in Level 1 V4.00 CALIOP products, the nighttime 532 nm channel is calibrated between 36 and 39 km using GMAO Met data. Please explain how the (nonattenuated) backscatter profiles are obtained from CALIOP V4.00 Level 1 data. Because CALIOP is a lidar instrument operating at the same wavelength as LTA, it could make sense to have a dedicated section for CALIOP.

Section 3

Line 200: “zonally averaged”...I suggest to say here “ over a 10° latitude belt centered at OHP latitude” (line 217).

Line 204: Can you clarify whether the differences reported in Table 1 are computed for different time periods, depending on the availability of the 2 instruments that are being compared? If yes, these time periods could be specified. I could not find the definition of the differences reported in the table (column – row or the opposite?).

Line 229: “quiescent periods”: please explain, because the careful selection of the quiescent periods is presented later in the paper.

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Lines 228-234: I don't see a negative bias of the OHP lidars with respect to all satellites above 25 km in Figure 2. Indeed, it seems that OSIRIS has the smallest values. Do you agree? What is the relative difference between the OHP lidars and CALIOP above 25 km? Would it be consistent with the presence of aerosols at 30-33 km, which could be further estimated from CALIOP? Could the difference be due in part to the use of different models (NCEP vs model used in CALIOP algorithm)?

Lines 236-237: I am not totally convinced by the discussion about the lidar ratio, because this issue should not introduce differences between LTA and CALIOP. The notion of desaturation is difficult to understand without specific knowledge of the instruments. I suggest describing these instrument-related difficulties in Section 2.1.

Section 4

Lines 251-253: Table 2 should be introduced in the text. Do you mean $VEI=4$ or $VEI \geq 4$?

Lines 308-314: What are we learning from the fact that no plume from the Calbuco volcano (41°S) is detected at OHP? Why mention this volcano specifically?

Lines 316-333: are the volcanic plumes detected using global satellite coverage or by using the two conditions listed in lines 329-332? My understanding is that global satellite measurements are used to trace the origin of aerosol enhancements detected using the two conditions back to volcanic eruptions. Is it correct? Fig. 6 is complicated and described only in section 5, and introduced before Fig. 5.

Lines 355-356: would it be correct to say that that OHP lidars detect an increase in optical depth end 2010/early 2011, which is traced back to the Merapi volcano using CALIOP observations?

Overall, I think that Section 4 is not very clear and should be reorganized. It is noted that it starts with a long introduction from lines 248 to 314, followed by only one subsection (4.1).

Section 5:

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Line 443: please describe Fig 7c.

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