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Interactive comment

Interactive comment on "Effect of volcanic aerosol on stratospheric NO_2 and N_2O_5 from 2002–2014 as measured by Odin-OSIRIS and Envisat-MIPAS" *by* Cristen Adams et al.

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Response to Referee 2

We thank you for your comments, which have helped to improve our manuscript. Below we address the recommended changes point-by-point.

1 General Comments

A big problem with the paper is that it totally relies on monthly zonal means which is not appropriate for the rather local and short lived volcanic plumes. Because of this, MIPAS SO_2 *data are provided as 5-day means (Höpfner et al., 2015). Regarding the monthly zonal mean binning of AOD, smaller bins have been tested and found to have minimal* *impact for the OSIRIS measurements. Please see response to General Comment B in response to Reviewer 3 for further details. We have elected to remove MIPAS SO_2 from the analysis, as described in more detail in responses below. Several important volcanic events listed there are missing or placed at the wrong latitude (Figure 3).*

Volcanic events are missing because we have only listed events that were observed in the OSIRIS stratospheric aerosol measurements. We have clarified this in the figure caption:

"The yellow triangles indicate the volcanic eruptions that were followed by significant increases in OSIRIS aerosol extinction, as listed in Table 2."

We have also clarified how volcanic eruptions are identified in other parts of the text, as noted in the details of this reviewer response below.

Thank you for noticing the problem with the latitudes of the volcanic eruptions in Fig. 3. The latitude of Sarychev Peak has been corrected in the revised figure.

There is also no need to use inconsistent climatological ozone for the photochemistry, from both instruments, OSIRIS and MIPAS, are selfconsistent data available.

Uncertainties due to the use of these climatologies have been estimated and are described in the text (Sect. 2.3), as well as in Figure 8 of response to Reviewer 3.

2 Specific comments

Abstract: Do the authors mean a 4 km thick layer above the tropopause, in tropics and midlatitudes, and against what conditions is the change?

We have re-worded this part of the abstract as follows:

"OSIRIS profile measurements indicate that the strongest correlations between NO_2 and volcanic aerosol extinction were for the 5-km layer starting 3 km above the mean tropopause at the given latitude. OSIRIS stratospheric NO_2 partial columns in this layer were found to be smaller than baseline NO_2 levels during these aerosol enhancements by up to 60

Please define NOx in line 9 of page 2.

We have added the definition.

Please improve wording in line 13 of page 3, it contains contradictions.

We have reworded this to

“OSIRIS measures limb-scattered radiances from 82°S to 82°N, with nearly full coverage in the summer hemisphere.”

From the data version number, it looks like that Höpfner et al (2015) is used (line 9ff,page 4), here also the SO_2data prior to 2005 are OK. In these data, especially if the 5-day means are used, all important volcanic events should be identified with significance (see also lines 15 and 25, page 8). This is not the case for the older dataset presented in Höpfner et al (2013) which had the focus on the middle and upper stratosphere.

MIPAS SO_2 played a very minor role in this paper. We included it because it was interesting to see how the SO_2 VCDs varied in the context of the OSIRIS AOD time-series. Since we are using monthly averages for the other species in this study, we presented SO_2 in a consistent way. However, we don’t want misrepresent the resolution/capabilities of MIPAS SO_2. Therefore, we have elected to remove MIPAS SO_2 from the paper. This means that panel b of Fig. 3 has been removed and that we have deleted the paragraph describing the MIPAS SO_2 and replaced it with a reference to Hopfner et al. 2015. This has not affected any of the major conclusions/results of this paper.

For the crude assumptions on the Mie scattering efficiency the wavelength should be repeated (line 2, page 5). The statements on particle size (line 5, page 5) are confusing, more details please, give at least a range for the effective particle size. If you model particle size from aerosol formation from injected SO_2, you get in increase in effective *particle size for both volcanoes. What is the basis for the crude error assumptions (factor 3)?*

The basis for the large uncertainty we prescribe is two-fold: (1) We can only make an educated guess at the particles sizes and (2) since average particle sizes can increase or decrease following and eruption, depending on the relative amounts of new (smaller) particles formed vs. growth of existing particles. For a handful of eruptions there are some observations available, but most of those considered here this is not the case. This leads to uncertainty as to how to adjust the scattering efficiency. For (1) we conservatively estimated a factor of 3 based on sensitivity studies using different size parameters and distributions, and for (2) we estimated uncertainty based on Thomason et al. (1997). It is worth noting these uncertainties (presumably towards an upper limit) still do not have that large an impact on the model results, showing that the simulations are quite robust.

In response to the comment above we have fleshed this out with some additional detail/explanation.

"A scattering efficiency of 0.40 was calculated using Mie theory for background spherical sulfate particles (based on a log-normal distribution with size parameters of r_g=0.08 μm and σ_g=1.6). However, volcanic eruptions alter the size distribution, as SO_2 rapidly forms sulfuric acid, which can condense to form new small particles or increase the size of existing ones. This change in size distribution will affect the scattering efficiency, but the sign of this change is unknown. For example, two months after the Kasatochi eruption, there was a shift in the ambient size distribution toward smaller particles (Sioris et al., 2010) whereas Sarychev led to a shift toward larger particles (O'Neill et al., 2012)."

And

"To account for potential errors and variability over 2002-2015 in our background SA, we scaled 10000 by factors of 3 and 1/3. The large factor is based on the sensitivity of scattering efficiency to the aerosol size parameters for the particles sizes and wavelengths considered here. For example, a change in effective radius by a factor of 2 leads to a change in scattering efficiency by a factor of 3 (see Hansen and Travis [1974] Figure 8)."

Isn't there also an averaging kernel for OSIRIS (line 19ff, page 5)?

There are not averaging kernels for OSIRIS retrievals. The resolution of OSIRIS is 2 km at the altitudes considered here, which is sufficient to retrieve partial column VCDs for the 5-km altitude range.

In Fig. 1 an additional panel with the zonal wind at 20 km (?) might be useful (line 21, page 6).

We have not included the zonal wind at 20 km since the QBO principal components are derived directly from the zonal winds.

Why are different partial columns given in section 3.3 (line 23, page 7) and earlier in the text (including abstract)?

The same partial column ranges are given in all section. The partial columns are calculated for the 5 measurement layers between 3-7 km above the mean thermal tropopause (e.g., over a 5-km range). We have clarified this in the text in Sect. 3.1.

". . . a 5-km altitude range starting at 3 km above the mean NCEP thermal tropopause at each latitude"

In Table 2 at least the eruption of Rabaul in Oct. 2006 is missing.

There was not a significant increase in OSIRIS aerosol extinction following the Rabaul eruption and therefore it, and likely other volcanoes, are not included in this table. We have added the following to the table caption to make sure this is clear:

"Note that this table only include eruptions that were followed by significant increases in OSIRIS aerosol extinction. Therefore it does not include all volcanoes known to have affected the stratosphere during this time-period."

There appear to be contradictions between Fig. 3 and 4.

I cannot find these apparent contradictions. The data presented in these figures come from the same data vectors, etc. We have revised the colorbar of Fig. 3 in order to make it easier to compare.

Improve Fig. 3 concerning $SO_2$with the Höpfner (2015) 5-day dataset. We have removed MIPAS SO_2 from the figure and the paper, as described above.

Fig. 3: Place the symbols for the volcanoes at the correct latitude.

The latitude for Sarychev Peak has been corrected.

It might be better to use volume mixing ratios at 19 km (or 3 km above the tropopause) instead of the partial columns to reduce data gaps.

In earlier iterations of this analysis, we did consider VMRs at fixed altitude levels above the tropopause, but elected to present the results as partial columns. We have added the following text to the first paragraph of Sect 3.1 in order to explain this:

"Partial columns were used, instead of, e.g., volume mixing ratios at a fixed altitude, because the largest observed aerosol extinction ratios related to volcanic aerosol were observed at different altitude layers for different latitudes and times. The partial column altitude range was selected to include most of these large extinction ratios. Furthermore, MIPAS NO_2 measurements have low resolution at the altitudes affected by volcanic aerosol and therefore are better presented as partial columns."

The results are also sensitive to the treatment of negatives in the individual data.

We did not remove negative values in the individual profiles before averaging because we did not want to create a high bias in the datasets when averaging values that are close to zero.

I don't understand the statement on tropospheric water vapor (line 22, page 8). The current understanding is that for explosive eruptions SO_2 is directly injected into the *stratosphere, in the plume only water from the volcano might matter, but the satellite sees only what comes out of the plume.*

Since we have removed SO_2 from the paper, this has been deleted.

Section 4.2: In Figs. 1, 6 and 7 appear often extinction ratios < 1. Please explain or correct, from definition this should not happen. Please adjust color bars to reasonable range,

The extinction ratio is defined as (Aerosol Extinction / Raleigh Extinction), in which case if the aerosol extinction is greater than the Rayleigh extinction, the ratio will be greater than 1.

We are also unsure what the reviewer means by "reasonable range". Therefore, we have adapted the color bars on various figures using the recommendations of Reviewer 1.

Don't use formulations like 'somewhat linear' (line 4, page 10; line 3, page 12).

This has been corrected, as per response to Reviewer 3.

3 Technical corrections

Line 20ff, page 1: Better 'anticorrelation' instead of 'relationship'.

This has been changed

Line 6, page 5: typo and bad wording.

We have reworded this paragraph, with the text starting with "In order to remove the seasonal variation from the NO_2 time series, the NO_2 anomaly (dNO_2) was calculated for each bin of the monthly mean NO_2 VCDs…"

Figure 3, caption line 4: Do you mean a 4 km thick layer 3 km above the tropopause? Please improve text.

This has been changed to "5–km layer starting 3 km above the thermal tropopause"

Truncate Figs. 6 and 7 at 12 km, the data below are not relyable. Say 'aerosol extinction ratio' also in captions. The black contours are superfluous. The colorbars should have the same steps as the colors in the figures (less is more!).

We have changed "extinction" to "extinction ratio" in the captions and the figure color bars and contours have been changed.

Fig.8: Caption: Say 'correlation coefficient' instead of 'R'. The colorbar should have the same steps as the colors in the figure.

The caption and figure have been changed as recommended.

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