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Discussions

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 1

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

The manuscript presents interesting and novel results, but important issues should be addressed before publication. The main one, in my opinion, is that the analysis relies on averages calculated into monthly 10° latitude bins. Using such large intervals in time and space to study quite short-lived and local events such as volcanic plumes can lead to important biases in the results.

See response to General Comment B in response to Referee 3.

Specific comments:

p.2, l.9: "NOx" should be defined.

We have added a definition for NOx.

p.4, l.18: Please give more details on the climatological profiles used for ozone and temperature, and explain your choice of using these profiles rather than using directly MIPAS and OSIRIS measurements of O_3 and T.

This has been addressed in response to Reviewer 3 under Sect. 2.3.

p.4, 19-20: Please clarify the sentence "All remaining species are calculated to be in a 24-hour steady-state by integrating the model over 30 days, but fixed to a specified Julian day".

This has been addressed in response to Reviewer 3.

p.4, l.22: The effects of polar stratospheric clouds are not considered here. However, these could play a role in the altitude range under consideration, especially in summer, which is the season on which this study is focused. This should be discussed while interpreting the results.

The lack of PSCs should not be a large factor here since only latitudes north of 55S are considered in the updated analysis (see Response to Reviewer 3, General Comment A). Moreover, the intent of the model is to demonstrate that the 'average' relationship seen in the OSIRIS NO_2 and aerosol extinction observations can be captured by the model, thereby demonstrating that the two are consistent with our current understanding of the impact of aerosol on NOy partitioning. It is possible that one or more OSIRIS monthly/zonal means might be contain some PSC signal, and this would lead to some departure from the non-PSC expected relationship, but there is no evidence of this in the observations at 70° or 80°N and at the high end of the AOD scale (the last two panels in Figure 5).

p.5, l.9-10: How the factors 3 and 1/3, used to account for potential errors in your background aerosol surface area, have been chosen?

The relationship between surface area and extinction, for Mie theory, is through the scattering efficiency, which depends on several factors. The most important of is the aerosol size distribution. As there is some uncertainty in the parameters than define this, such as the effective radius and the variance of the distribution, we considered a range of different values. The end results was the scattering efficiency was found to vary by a factor of 2 (1/2), and so we used a factor of 3 (1/3) to be conservative in our uncertainty estimates

p.5, l.21: Place clarify "for the five measurement layers from 3-7 km above the NCEP thermal tropopause..." I do not clearly understand which measurement layers you are using.

We are adding the partial column using the layers 3, 4, 5, 6, and 7 km above the tropopause. We have clarified this throughout the text by instead referring to the partial column over "the 5-km layer starting 3 km above the mean tropopause at the given latitude"

Section 3: The analysis is based on monthly means calculated in 10° latitude bins. The choice of such large intervals in time and space does not seem to be the most appropriate to study volcanic plumes, which are quite short-lived and local events. A different distribution of the observations from OSIRIS and MIPAS in a given bin might lead to very different results. The same comment is also relevant for the photochemical model, which covers the whole bin in a uniform way, while this is probably not the case for the observations. Was it not possible to perform the analysis using more appropriate bins? In this case, the authors should find a way to estimate the sensitivity of their results to this problem, and this should be thoroughly discussed in the paper. Have the authors try to use only the modelled data in collocation in time and space with the

observations?

For OSIRIS and the photochemical model, there should not be great sensitivity to this. OSIRIS NO_2 and extinction measurements are sampled approximately the same. For the photochemical model, we are interpolating NO_2 to the mean extinction as measured by OSIRIS.

MIPAS sampling is not identical to OSIRIS, but covers the selected bins fairly evenly. This has been discussed in Response to Reviewer 3, General Comment B.

p.8, l.13-17 and Fig.3: Please comment the high AOD levels at southern high latitudes, which are obviously not due to volcanic eruptions. We can read, p.9 l. 5-6, that these are "perhaps due to polar stratospheric clouds". This should be discussed already in the description of Fig.3, and this statement should be explained, or at least associated with a reference.

We have removed southern hemisphere high latitudes from analysis, as discussed in General Comment A in response to Referee 3.

p.9, l8: Is the second interval considered to calculate the correlation coefficients 40°N-80°N or 40°S-80°N? The information given in the figure is inconsistent with what is said in the text. Same remark p.9, l.22.

Thank you for noticing these typos. We have corrected the text to 40°N-80°N.

p.9, l.16-18: This could be checked by applying MIPAS averaging kernels to the OSIRIS data. This has already been done in several studies comparing MIPAS data to other data sets characterised by a better vertical resolution.

This has now been tested. See response to General Comment C in response to Referee 3.

p.10, l.15-16: Was the vertical resolution of the MIPAS N_2O_5 profiles also not good enough to look at the effect of volcanic aerosol on this species as a function of altitude?

The vertical resolution of N_2O_5 is 6-8 km for much of the lower stratosphere (see Fig. 3 of Mengistu Tsidu et al 2004) and therefore is not sufficient to look at the effect of volcanic aerosol. We have clarified this in the text in Sect. 4.2:

"The MIPAS NO_2 and N_2O_5 profiles were not considered because of limitations in the vertical resolution in the lower stratosphere."

Technical corrections:

p.1, l.24: Remove "of"

This has been removed

p.1, l.25: Please change "relationship" to "anti-correlation"

This has been replaced

p.3, l.22, p.4, l.1, p.4, l.14 for example: Please write "lower than" or "greater than" instead of "<" or ">".

We have replaced the ">" and "<" throughout the document.

p.5, l.19: For the sake of clarity, please add "number densities" after "SO_2, NO_2, N_2O_5 and HO_3".

This has been added

p.6, l.13: "on the same order of magnitude AS the variation..."

This has been corrected

p.6, l.27: "...periods that WERE not affected by volcanic aerosol."

This has been corrected

p.10, l.17: Please add "percent difference" in "OSIRIS and modelled NO_2 percent difference profiles".

This has been corrected

p.10, l.20-23: For the sake of clarity, the given percent different values should be negative (same remark p.11, l.2)

We have specified all the percent differences as negative, as recommended

Fig. 3, 6, 7 and 8: These figures would be clearer if there was a limited number of colours in the colour bar, so that it corresponds to the colour levels shown in the plots.

We have changed the figures as recommended.

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