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Atmospheric Measurement Techniques Discussions

Interactive comment on "Stratospheric Extinction Profiles from SCIAMACHY Solar Occultation" *by* Stefan Noël et al.

Stefan Noël et al.

stefan.noel@iup.physik.uni-bremen.de

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Reply to referee 2

We thank the referee for the overall positive judgement and will consider the comments in the revised version of the paper. In the following, the original reviewer comments are given in *italics*, our answer in normal font.

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Discussion paper

Answers to comments:

1. *One of the bigger issues, which is already brought up the other reviewer, is the low frequency vertical oscillation of the profiles. This could limit the usability of the data set, and the authors make a rather sweeping assumption that this is due to the nature of the onion peeling method without regularisation. I strongly recommend further work to track down and potentially improve the oscillatory behaviour; if it is as simple as adding regularization to the retrieval, then it certainly should be done.*

   A regularisation is not possible for the current onion peeling method as it would require the coupling / simultaneous retrieval of different tangent altitudes (including lower ones).

   For further information, we repeat here our answer to referee 1:

   We agree with both referees that the vertical oscillations are the most critical issue for the SCIAMACHY solar occultation data product. This is why we explicitly mention it e.g. in the conclusions. These oscillations are not only a problem for the extinction retrieval but also for the greenhouse gas profile retrievals published in earlier studies. We have investigated this issue for several years, but could not identify the reasons for these oscillations. We assume they are caused by a deficiency in the radiometric calibration in combination with the onion peeling method as they seem to appear at all wavelengths. The only way to handle these in the current algorithm is to apply an additional vertical smoothing of the profiles, which we do for trace gas profiles using a boxcar of 4.3 km width. The value of 4.3 km is chosen, because this corresponds to the approximate vertical range of one readout (combination of size of instantaneous field of view and scan). We could choose a larger smoothing width here and/or apply additional smoothing to the extinction / transmission profiles. Since the oscillations have a period of about 10 km, we would need a smoothing width of at least this size, which would

result in a data product with a very low vertical resolution (only ∼2 independent data points). We decided not to do this, as this can still be done by data users if required for a specific purpose.

Note that these oscillations become less prominent (amplitudes <10%) when comparing with data sets where more collocations covering longer times are available (e.g. SCIAMACHY limb and also the newly included OSIRIS data). This averaging effect indicates that sampling and statistics also play a role here.

Our solution to overcome the problem of vertical oscillations is to use anomalies for scientific studies (as we do in the paper). In these anomalies systematic effects – including the oscillations – are essentially removed while keeping the vertical resolution.

We will explicitly include this in the abstract and conclusions of the paper.

2. *The other issue is the reported linear changes that are derived from the time series. The nature of the time series is highly variable due to the volcanic perturbations as nicely shown in Fig 14. The linear analysis is simply not justified. Yes, you can fit a straight line to this, but to do so is not justified, and then to report a "significant positive change of 20–30% per year" is somewhat misleading. Here the comparison with the SCIAMACHY limb scattering retrievals is quite interesting and reasonable, but with differences that the authors claim are due to different measurement times and locations. It would be better to put effort into understanding these differences and skip the linear analysis.*

   We agree that due to the volcanic eruptions the temporal evolution of extinction is indeed not linear. We only use a linear model to somehow quantify this change (we explicitly do not call it a trend). We also explicitly mention, e.g. in abstract and conclusions, that the changes we derive at lower altitudes are due to the volcanic eruptions. The numbers we give are therefore very specific for our data set. Nevertheless, we think they are a useful result which should be mentioned.

In order to avoid misunderstanding, we will clarify that these numbers should not be interpreted as a continuous trend.

Regarding the differences to limb, we will add some further explanation, i.e. that limb observations are affected by changes in particle size that might have an influence on the resulting linear changes.

3. *Other smaller issues: Every time that an agreement between observations is claimed to be "good", please be sure to quantify.*

   We will check this and add information where required.

4. *Several times in the paper, the authors refer to the "extinction", where usually it is referring to the aerosol extinction. Please include this each time as extinction is a generalized quantity in radiative transfer and does not just refer to aerosol.*

   Agreed. We will update the text (and the title) accordingly.

5. *The statement at the bottom of page 2 that occultation measures extinction "whereas" limb sounders are more sensitive to smaller particles need qualification. Please explain in more detail. Do you mean limb scattering? In general limb scattering is definitely sensitive to large particles.*

   The sensitivity of limb measurements to smaller particles refers to the particle size distribution, which is not dealt with in this paper. We will delete this part of the sentence to avoid misunderstanding.

6. *For SAGE II comparisons, why include the time criteria of 9 h if it doesn't matter? Also, "temporal distance"is not a standard phrase; "time difference" is clearer.*

   Agreed, 9 h is indeed no limiting factor. We will reformulate this and also replace "temporal distance" by "time difference".

7. *The explanation of figure 3 needs to be clarified on page 5. What is the numerical sun shape function, S? For a localized aerosol or cloud layer, the transmission will*

have a perturbed shape. How would this be handled by the algorithm to derive the shape function?

The sun shape function S refers to the sun shape without atmospheric influences. It is determined from the reference measurements at higher altitudes (around 90 km) where influences of aerosol and clouds can be neglected. We will clarify this in the text.

8. *What is the impact of choosing second order for the polynomial in the fit to the transmission spectra?*

   Since the fitting windows are usually not that large it is sufficient to use a small order polynomial to describe the background signal. However, as long as there are no spectrally broadband absorption features also higher orders do not change the results very much. We tried several orders, and second order seemed to be most appropriate in our case.

9. *Table 1 lists SCIAMACHY and OMPS nadir modes. These are not used for stratospheric aerosol to my knowledge.*

   In the table we listed all measurement modes (even if not used for stratospheric aerosol retrieval). We will update Table 1 and mark those modes which are not used for stratospheric aerosol retrieval for clarification.

10. *Figure 3 caption uses the word "spectra" for the figure. These are not spectra.*

    Yes, this wording is wrong. We will correct this.

11. *Figure 6 caption should explain the terms in the fit.*

    Will be done.

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