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

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Reply to referee 1

We thank the referee for the detailed review. The comments will be considered in the revised version of the paper. In the following, the original reviewer comments are given in *italics*, our answer in normal font and the proposed updated text for the revised version of the manuscript in **bold** font.

Answers to General comments:

1. *As pointed out by the authors themselves, the ONPD retrieval algorithm leads to oscillations in the retrieved extinction profiles. This had been noted earlier in the retrieved profiles of gas species as well by the same authors and yet no effort has been made to ameliorate this issue. From the comparison of an individual profile with SAGE II (Fig. 8) it would appear that the oscillations are largely at altitudes over 30 km where the aerosol extinctions are very low anyway. However, later the oscillations showed up in the statistical comparison (Fig 9) at pretty much all altitudes. These oscillatory profiles make the data product of limited value. I think the paper would improve significantly by addressing this issue.*

   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 $\sim$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, see next point). 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. *It will be useful to include intercomparison with some other concurrently available data products. The authors could explore using SAGE III on Meteor-3M or POAM III. In particular, the limb scatter data from OSIRIS provides good coverage spatially and temporally. The newly released level 3 stratospheric aerosol product from CALIPSO lidar also covers from ∼80° S-80° N and has good overlap in time with SCIAMACHY between 2006 and 2012. Inclusion of some of these intercomparisons will a add value to the paper.*

   Thank you for the suggestions. We will include comparisons with SAGE-III and OSIRIS in the paper.

Answers to Specific comments:

1. *Page 2 line 25: The indirect effect of aerosols on the clouds may be more relevant in the troposphere or do you mean the overshooting clouds or the cirrus clouds near the tropopause?*

   This was a more general statement. We agree that the indirect effect is especially important in the troposphere. In the stratosphere, the indirect effect is more

related to generation of e.g. PSCs, which is mentioned in the following sentence. To clarify this, we will reformulate this part as follows:

   Stratospheric aerosols play a important role in climate as they affect radiative forcing either by scattering and absorption of light (direct effect) or by their impact on clouds and ozone (indirect effect). Especially, aerosols affect the creation of polar stratospheric clouds (PSCs) on which surfaces O_3 depletion takes place.

2. *One solar occultation instrument missing in the introduction as well as in Table 1 is MAESTRO on board the Canadian SCISAT mission, e.g. see McElroy et al. 2007, Sioris et al., 2010. Also, in Table 1, please add the latitude range covered by each instrument.*

   We will add MAESTRO in the table and the text (thanks for the references). Also, latitude ranges will be included in Table 1.

3. *Page 2, line 48: It is probably fair to mention clearly that CALIOP is different from the other instruments listed in Table 1 because it is an active remote sensing instrument. It is primarily intended for tropospheric aerosol extinction measurements although stratospheric aerosol extinction retrievals have been recently produced. More relevant references for these stratospheric measurements by CALIOP are Thomason et al. (2007) and Kar et al. (2019).*

   We will mention this in the introduction and include the references.

4. *Page 3, line 73: What do you mean by "actual" pressure and temperature profiles? In fact I am wondering why the authors used ERA-Interim rather than the newer ERA5 reanalyses. Are the pressure and temperature at mid-high latitudes in ERA-Interim better than ERA5?*

   "Actual" just means that we use the pressure and temperature profiles closest to time and place of the measurement. We will clarify this in the text.

Our data product was created before ERA5 was released, therefore we use ERA-Interim data. ERA5 data have a higher spatial and temporal sampling that ERA-Interim, therefore they should indeed provide better information. However, we expect the impact of changing to ERA5 on the occultation products to be small compared e.g. to our assumption of a linear temperature correction. Especially, this would not solve the oscillation problem (see above).

5. *Page 5, line 122: Please delete "exemplary" and rephrase this sentence.*

   We will reformulate this sentence accordingly:

   The right plots of Fig. 3 show this varying signal for the reference scan at high tangent altitudes, where atmospheric absorption and refraction are small and can be neglected.

6. *Page 6, line 148: Please first refer to Figure 4 before this sentence.*

   Will be done.

7. *Page 7, line 205: Why is 4.3 km used as the width for box car averaging? What is the impact of using a different choice on the vertical oscillation problem? Some discussion of this issue is needed here.*

   4.3 km is the approximate vertical range covered during one readout (see above). We will clarify this in the text and add some discussion.

8. *Page 9, line 240: Please mention the coincidence information between the two measurements for this case, including the latitude and longitude.*

   Will be included.

9. *In Fig. 8, there is a large difference between the SCIAMACHY occultation and SAGE II profiles at the lowest altitudes (10–12 km) — could this be due to cloud related effects?*

   As mentioned in the text, the current SCIAMACHY occultation data below about 15 km are not considered to be reliable. This is in general due to tropospheric effects, which includes clouds but also strong changes in e.g. temperature gradients which are not resolved by the instrument because of the $\sim$4 km resolution. One purpose of Fig. 8 is to show these limitations.

10. *Page 10, line 295: For completeness, please mention how the differences with SAGE II extinction profiles were calculated in the text, although it is given in the legend to the Fig. 9. Also please mention if any filtering criteria were used.*

    We will describe in the text how the differences are calculated. We did not use specific filters, only removed those altitudes which are marked as invalid in the data (usually the lowest ones).

11. *Page 10, line 301: Do the results change by tightening the coincidence criteria?*

    Specifically for the SAGE-II comparison the number of collocations is not that large, therefore we prefer not to tighten the criteria here. However, we have checked that even with a reduced number of collocations with SAGE-II we get essentially the same results.

12. *Page 11, line 304: By "mean error", do you mean the standard error of the mean?*

    With "mean error" we refer to the mean of the error given in the product. We will clarify this in the text.

13. *In Fig. 10, there seems to be a bias in the background case, the agreement is good mostly between 20 and 25 km with significantly larger biases above and below this altitude range.*

    Yes, this is true, We will update the text accordingly.

14. *Page 11 and line 320: Why do the size distribution issues affect only low altitudes? Please discuss.*

The impact of the volcanoes changes the size distribution of the particles. This is mainly limited to the lower altitudes because perturbations in the particle amount and their sizes due to volcanic eruptions rapidly decrease with the altitude. Perturbations due to volcanic eruptions usually do not reach above 20 km in the period from 2002 to 2012.

We will explain this in the text.

15. *What are the black vertical lines in all the panels in Fig. 11?*

    These (grey) lines mark times of degraded instrument performance (like decontamination period or switch-offs). We will mention this in the caption.

16. *Page 12, line 339: Note that the volcano Nabro occurred at low latitude (13°N) and the aerosol plumes spread later to higher latitudes.*

    We will update the text accordingly.

17. *Page 13, lines 374-376: I think the interpretation of the anomalies at altitudes above 25 km in terms of QBO is an interesting result that needs to be discussed further, rather than simply assuming it to be the case. Please add a plot of a QBO index on top of the panels in Fig. 12 so the correlation between the aerosol anomaly and the QBO can be seen more clearly and then discuss the observed anomalies at middle/high latitudes for the easterly and westerly phases of QBO and in terms of aerosol transport from the tropics. Also please discuss the effect in terms of altitude.*

    A detailed discussion on transport effects and QBO is included in our water vapour / methane paper (Noël et al., 2018). This also includes a plot of the QBO index. We do not want to repeat this full discussion in the present paper, but make a reference to this paper and a related one focusing on SCIAMACHY limb data (Brinkhoff et al., 2015, see below) with a short summary and add the QBO index in Fig. 14 (together with the time series at 25 km) for clarification.

Brinkhoff et al. (2015):
Ten-year SCIAMACHY stratospheric aerosol data record: Signature of the secondary meridional circulation associated with the quasi-biennial oscillation, in: Towards an Interdisciplinary Approach in Earth System Science (p. 49–58), Springer, Switzerland, https://doi.org/10.1007/978-3-319-13865-7_6

18. *Do the linear trends shown in Fig. 15 conform to trends from other studies, if any?*

    We do not know of other extinction studies covering the same time, altitude and latitude range. In any case, a comparison of changes would not be straightforward because of the specific spatial/temporal sampling of the SCIAMACHY data.

19. *Page 14, line 413-414: Is the QBO effect expected to be similar for gas species and aerosols?*

    Yes, we see similar effects in our greenhouse gas products, see above.

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