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

Anonymous Referee #2

Received and published: 9 June 2020

The authors present a new retrieval of stratospheric aerosol extinction profiles from SCIAMACHY solar occultation measurements. This has potential to be a useful data set as satellite observations of stratospheric aerosol, while important, tend to be plagued with issues due to their tenuous nature and the information poor nature of the observations. Even with the limited latitudinal sampling of the SCIAMACHY solar occultation measurements, these are valuable as the occultation approach does not have to rely on the assumptions about particle size distribution/concentration or composition that are required for lidar or limb scattering. The paper describes the retrieval approach, a preliminary "validation" of the results and some interpretation of variability in the multi-year data set. The work is well suited to AMT, however there are few issues with the paper that should be addressed before publication.

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.

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.

Other smaller issues:

Every time that an agreement between observations is claimed to be "good", please be sure to quantify.

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.

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.

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.

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?

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

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

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

Figure 6 caption should explain the terms in the fit.

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