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Atmospheric Measurement Techniques Discussions

Interactive comment on "SCIAMACHY stratospheric aerosol extinction profile retrieval" *by* G. Taha et al.

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We thank the anonymous reviewer for the detailed comments that helped improve this paper. Our responses for specific reviewer comments are produced below.

General comments

I suggest changing the title: it only points to the part of the paper that discusses the possibility for aerosol retrievals from SCIAMACHY. The possibility to do the same for OMPS is not mentioned, but this is the primary goal of the study.

We change the title to "SCIAMACHY stratospheric aerosol extinction profile retrieval using OMPS/LP algorithm"

Specific Comments

First a general remark: the wavelengths that are mentioned in the paper are not consistent. See for example fig. 8. The figure legend mentions 682 and 794 nm, while the caption mentions 692 nm and the text mentions 793 nm. Other errors can be found elsewhere. Please check and correct everywhere.

Corrected.

Page 5345, line 14: the author mentions that no solar occultation instrument is currently active. Perhaps he can cite GOMOS on Envisat, a stellar occultation instrument that has been active for ten years now and that delivers aerosol extinction profiles.

Change made.

Page 5346, line 20: 'The a priori vector is a set of mean constant extinction vertical profiles . . . '. This is not clear to me. Mean of what? Also, to me, a constant extinction vertical profile is a profile that doesn't change with altitude. This is probably not what the author meant. Please clarify.

The above sentence is replaced by "The a priori vector is a set of extinction vertical profiles (8 wavelengths) corresponding to the present period of low stratospheric background aerosol constructed from SAGE III measurements, and is fixed for all locations."

Page 5346, line 21: 'The a priori aerosol size distribution . . .0,06 μm effective radius and variance of 1.73 . . . index of refraction of m = 1.448 + 0 i' and following. Please specify where these values come from and why they were chosen. Also, do these values correspond (through an optical model, such as Mie theory) with the a priori extinction mentioned above? Finally, a lognormal distribution is not expressed as function of effective radius, but as a radius that represents the median of the distribution (sometimes called a 'mode radius'). Please correct.

The above sentence is replaced by "The a priori aerosol size distribution is assumed to be a background stratosphere aerosol of a single mode log normal distribution, with

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0.06 μm mode radius and variance s of 1.73, composed of spherical liquid sulphate particles with an index of refraction m=1.448+0i (Yue and Deepak, 1983, Wang et. al., 1996)." As stated above, these values were selected from the literature.

Page 5347, line 21: a normalization value at the altitude of 35.5 km is chosen, probably because the aerosol contribution is negligible there. Please mention this.

Change made.

Page 5347, line 24: 'A median Angstrom coefficient . . .': I guess this means the median of the angstrom coefficients evaluated from the retrieved extinctions at the different wavelengths. Please specify this.

Change made.

Page 5347, line 28: Please briefly explain what the RSAS channel is exactly. We added the following "...,usually at $\sim$350 nm, where the Rayleigh scattering is dominant and gaseous absorption is at minimum,.."

Page 5348, line 1 – 4: Please give a citation to a paper explaining the method using the moments of a size distribution.

Done.

Page 5348, line 5 - 10: 'Figure 1 . . .' Please specify that this figure was calculated for SCIAMACHY as is mentioned in the figure caption.

Done.

Page 5348, line 15: 'This data set was generated by a forward model . . .': Is it the same forward model as the one used in the OMPS/LP retrieval algorithm? Please specify.

Change made.

Page 5348, line 17: '. . . over a one-year period'. Do you mean colocations were

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searched for within a one year period? Please clarify.

Change made.

Page 5348, line 21: Surface albedo is assumed to be 0.15. Where does this value come from?

We added “which represents a clear sky conditions” to the sentence.

Page 5348, line 25 and following: discussion on Fig. 3. What is shown? True – Retrieved, or Retrieved – True?

The retrieval – truth, we added this to the text.

Page 5349, line 14: ‘To ensure maximum sensitivity towards aerosol, we use a very small aerosol a priori’. This doesn’t make sense. The author probably means a small aerosol a priori weight, which corresponds to a large a priori variance. Please correct.

To clarify the sentence, we replaced the last part with “..,we use initial guess profile of a very low aerosol loading.”

Page 5349, line 18: ‘Aerosol modeling error contributed up to 2% of the retrieval precision . . . since there was no attempt made to constrain SAGE II aerosol profile spectral behaviour by an aerosol size model’. How did the authors determine this error? For this one needs to know the ‘true’ spectral behaviour..

We replaced the last sentence with “In a separate test (not shown here) where the truth aerosols profile spectral behaviour is constrain by an aerosol size model, this bias disappears. ”

Page 5350, line 25: ‘the proxy data set is evenly distributed around the globe’. Not really, no location can be found above 50_N and below 70_S.

The above sentence is replaced by “the proxy dataset is distributed around 50 N and 70 S”

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Page 5351, line 7: 'evidence of straylight in the spatial dimension'. This sentence is confusing. I would suggest 'evidence of straylight at high altitudes'. Also: please explain why the contamination is stronger at long wavelengths?

We changed the text. We did not want to expand on the details of SCIAMACHY straylight as it can be can be found in the provided reference, (Gottwald et al., 2006).

Page 5351, line 11-16: The authors describe how they continuously alter the normalization altitude. I only would like to mention that this is a risky thing to do. Eventual bias in the retrievals will be dependent on whatever causes the straylight, and it will be more difficult to identify the cause of the bias because it changes all the time.

The reviewer point is valid as we already pointed out in the text (section 4.4). Unfortunately, in absence of any straylight corrections, this is the best way to minimize the straylight contamination.

Page 5351, line 18 – 24: Discussion on retrieved aerosol extinction. What do you use as first guess for the iterations? Please specify. Also, elaborate some more on the retrieval limits. Are the retrievals not updated during the iterations outside these limits? How are the limits determined?

The modified text describes the a priori, which is same as initial guess in (section 3). The aerosol limits are decided by the aerosol sensitivity or kernels as mentioned in section 2.2. We added the following to section 2.2 "The retrieval is not updated outside these limits."

Page 5351, line 25 and following. Explain why these two separate retrievals were performed (which purpose).

Change made.

Page 5352, line 1-4: first only ozone is retrieved with aerosol fixed to climatology, then ozone and aerosol are together retrieved. So we have two different and completely independent retrievals, of which I don't see the point. There is probably something

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missing here. Do you use the first ozone retrieval as a priori for the second? Please clarify.

As we said in the text, the second aerosol retrieval is performed with more accurate ozone to improve the shorter wavelengths retrieval, which are more affected by the ozone initial assumptions. We modified the sentence to clarify this point.

Page 5352, line 12: '. . . 513 – 682 nm interpolated results.' The retrieval is performed for 750, 793 and 1028 nm. One cannot interpolate these values to wavelengths outside this spectral range, the most we can do is extrapolate them. But this is probably not what is done. Probably the 513 – 682 nm values are evaluated with the Angstrom law, of which the coefficients are obtained from the retrievals. Please clarify.

That is true, the text is corrected.

Page 5352, line 14: 'dashed line': I don't see it on the figure? Also: '. . .interpolated SAGE II'. How was it interpolated. Linearly? Cubic? Spline? Please clarify.

The text is modified by adding "linearly interpolated".

Page 5352, line 19: 'SCIAMACHY aerosol profiles show a good agreement with SAGE II . . .'. This is a matter of taste. Personally I don't consider a bias of 25 % good agreement. I would prefer the term 'acceptable'.

We believe 25% is considered good for aerosol comparison given all the difficulties involved in such comparison and compared to other instruments documented performance.

Page 5353, line 8: '. . . 192-194 K' around the observed aerosol layers. Specify at which altitude. There are multiple layers visible. Also: 'the time, location, and temperature indicate that this layer is . . .'. The magnitude of the extinction is also an indication: a value larger than 10e-2 km-1 is much larger than what we expect from background aerosols.

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Change made

Page 5353, line 11: '. . . to detect a secondary aerosol layer . . .'. Specify at which altitude.

Change made.

Page 5353, line 21: '. . . a good agreement with SAGE II'. Also here I would prefer the word 'acceptable'.

As we discussed above, we believe it is good agreement.

Page 5353, line 24: '. . . which reflects on the larger variability of the differences for the compared profiles'. I don't understand this sentence. What is the author trying to say? Please rephrase.

We change the sentence to "which reflects on the larger variability of the observed differences"

Page 5354, line 15: 'some of the observed differences were real, caused by instrument differences and . . .'. Once again, I don't understand this sentence. Why are the mentioned differences real while others aren't? Please rephrase.

The text is changed to "Some of the observed differences were caused by instrument differences and atmospheric variability or mismatches, which requires further work to adequately account for."

Page 5354, line 27: '. . . high altitude biomass burning'. There is no such thing. There are however high-altitude clouds as a result from biomass burning. Please rephrase.

Change made.

Page 5355, line 15: 'The 1-sigma uncertainty is very small compared to the standard deviation, mainly because SCIAMACHY reported errors were small'. What do you mean? Are they too small to be realistic? Also: natural atmospheric variability explains

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partially the difference between sample standard deviation and retrieval uncertainty.

The reported errors are too small, not sure if I can comment on its quality. We change the text as suggested.

Page 5356, line 2: . . . deviation better than 15 % for the altitude range of 15 – 30 km.

Done.

Technical corrections

Changes were made to the text and the figures to address all the suggestions made by the reviewer under technical corrections. Thanks again for the detailed comments and corrections.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5343, 2010.