

Interactive comment on “SCIAMACHY stratospheric aerosol extinction profile retrieval” by G. Taha et al.

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

General Comments

various atmospheric conditions. However, the main motivation, retrieval of stratospheric aerosol extinction coefficient profile to better account for the aerosol effects in ozone profile retrieval process is left mostly untouched, with only one vague reference to it in the text.

We have shown in section 3 the aerosol retrieval is accurate and results in ozone
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density profile that is within $2 \pm 3\%$. The absence of any aerosol account in the ozone retrieval can result in up to 7% bias. In section 4.5, the difference between our ozone retrieval and the IUP retrieval showed an improvement of 3% in the UP/LS region, which can be explained by retrieving the aerosol extinction profile instead of using a constant climatology. The following sentence is added to the end of section 3. “Rault and Taha (2007) showed that ignoring aerosol contribution would produce an ozone density bias of up to 7% in the region of maximum aerosol extinction, while the use of constant aerosol climatology can introduce a systematic bias and a possible seasonal or geographical signature in the ozone retrieval around the aerosol maximum.”

Specific Comments

The algorithm is based on optimal estimation, which uses non-linear minimization to estimate the model parameters and their posterior uncertainties given the forward model and parameter prior specifications. The prior for OMPS/LP algorithm is described in page 5346, Sec 2.1, line 15 onwards. The sensitivity of the results to the selected prior is not discussed at all. Also, it seems that the term “a prior vector” is sometimes used as the initial value for iteration and sometimes as mean for the prior distribution. E.g. at page 5351 Sec. 4.2 line 4, and page 5349 Sec. 3, line 15 “a very small aerosol a priori” seems to refer to the initial value of optimization iteration. In general, details related to numerical methods should be distinguished from the statistical model used. Never the less, if the method is sensitive to the initialization, it should be a cause of great caution.

The retrieval is not sensitive to the a priori profile, (which is the same as the initial profile), as shown in Fig 8, and section 4.2. The a priori referred to in 5351 Sec. 4.2 line 4 is what is used in the retrieval and is only shown here to illustrate the independence of the retrieval on the a priori. We also replaced the term ‘a priori’ with ‘initial guess profile’ in couple of places.

Is the same forward model used in simulating the measurements and in the retrieval

algorithm? Is so, this "inverse crime" typically result much too optimistic uncertainty estimates as it underestimates the bias related to modelling error and other approximations in the modelling assumptions.

The forward model is basically the same as the one used in the inversion retrieval; however, for the forward model run, we use a higher wavelength resolution, as well as a finer single and multiple scattering grids than those used in the inversion forward model. We understand the reviewer concern, which is exactly why we also use SCIAMACHY measurements for our analysis. Change made to the text to explain this point.

Text is missing discussion on how well the real characteristic of actual OMPS measurements can be reproduced from SCIAMACHY measurements by the reconstruction outlined in Section 4.1

We only used a simple spectral and spatial interpolation, convolved by OMPS band-pass, as stated in the text. No attempt was made to reproduce any other OMPS characteristics as it require far more work that is beyond the scope of this work.

I would be more at ease with the term "difference" than with "bias". In the synthetic data case we are retrieving known profiles and we know the truth. The bias of the estimation algorithm could then be estimated by repeating the same retrieval with repeated sampling of the noise term. In the text the mean difference is wrt. the selected scenarios and will contain other sources of uncertainties than the bias in the classical sense. In the SCIAMACHY radiance measurement case the SAGE II profiles are assumed be accurate, but then the temporal and spatial difference in co-occurrence will still cause large differences to the results that are not related to the bias in the strict sense (as noted by the authors, also).

The term bias is changed to relative difference as suggested.

The term "mean bias" is also troublesome (Fig. 13, for example). Bias is usually defined as mean difference of the estimator to the true value, so it already includes

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taking the mean. Unless the term refers to the bias wrt. the mean in contrast to the bias wrt. median, for example. I would prefer "mean difference", still.

We replaced bias with difference.

The only place where the primary goal of the study (as stated in the Abstract) is assessed is at page 5355, Sec 4.5, line 13: "which could be attributed to an improved aerosol profile solution", where it is referring to Fig. 14. More elaboration on this matter would be in place.

We added the following sentence "IUP retrieval uses constant aerosol extinction climatology to correct for the aerosol contribution, which can introduce a systematic bias in the ozone profile depending on the time and location of the measured profile." We also added more discussions in section 3 as stated in earlier.

More comments:

page 5345, Sec 2.2, line 21: aerosol size distribution is mentioned here but in the rest of the text very little is said about the possibility to infer about it.

That is correct. As we said in the text, except for the Angstrom coefficient, no attempt was made to infer the aerosol size distribution in this work, and it remains subject to future work and separate study.

page 5347 Sec 2.1 line 1: The last sentence tries to explain how the uncertainties are calculated and how noise is added to the simulated reflectances(?). Maybe you should be more specific to make this more clear.

The sentence "No noise or instrument errors are added to this data set." was added to section 4.3 to clarify this point.

page 5347 Sec 2.2 line 21 "maximum likelihood" -> "maximum a posteriori" if real priors are used in the optimal estimation.

As stated in the text, we are using a climatology as an a priori.

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page 5349 Sec. 3 line 27 "It is clear" please be more specific, "Because of ..., it is clear..."

we added to the text "Since the ozone retrieval is within $2 \pm 3\%$ of the truth,'

page 5356, Sec 5, line 4: isn't there a danger that fine-tuning an algorithm to give results that agree too well with some specific external data set would induce additional bias?

We believe that the use of simulated radiances can prevent any introduction of such biases, since the truth is always known. Any fine-tuning must pass the simulation test too. We are also using other instruments measurements for our algorithm development (not presented here).

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 comments and corrections.

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