

## ***Interactive comment on “BrO vertical distributions from SCIAMACHY limb measurements: comparison of algorithms and retrieval results” by A. Rozanov et al.***

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Dear Referee,

thank you very much for your comments. Please find our detailed replies below.

*"The treatment of the measurement error is not properly discussed. The concept of posterior error is used but it is not defined how it is estimated. I suggest that the error propagation (how the measurement error and modeling errors are propagated through the inversion steps and how the "theoretical precision" is computed) is added."*

We have added a formula explaining how the theoretical precision is computed. Propagation of modeling error is not included in the precision and is discussed in Sect. 7.

*"I suggest that a statistical comparison of the BrO retrieval of the three different algorithms is included. This could be done e.g. by using data from one orbit or something like that. It would be important to see if there are some systematic differences in the algorithms (eg. latitude or altitude dependent)."*

We have added an intercomparison of different SCIAMACHY BrO data products for one orbit.

*"I would like to see some discussion how the BrO retrieval algorithms compare with the NO<sub>2</sub> and O<sub>3</sub> retrieval algorithms that are applied to SCIAMACHY data: similarities and main differences. This could be used to motivate the manuscript."*

We think, because of a high scientific importance of bromine, BrO retrievals does not really need to be motivated by comparisons to NO<sub>2</sub> or O<sub>3</sub>. Furthermore, as there are again three NO<sub>2</sub> retrievals which are quite similar to BrO retrievals and three O<sub>3</sub> retrievals some of which are similar to BrO and some of which are totally different, this discussion will significantly increase the length of the paper. We think it would just decrease the readability if we include an additional discussion which is not closely related to the topic.

*"P 5082 – L20: Whereas... – the sentence is unclear and too long."*

The sentence has been rewritten.

*"P 5083 - L15. Adding a figure of the measurement geometry would be useful."*

The requested figure has been added

*"P5086 – L1. There are also other techniques besides DOAS and global fit. Among the OSIRIS teams also so called Flittner algorithm (Flittner et al, O<sub>3</sub> profiles retrieved from*

*limb scatter measurements: Theory, Geophys. Res. Lett., 27, 2601–2604, 1999) and a so called Modified Onion Peeling algorithm (Tukiainen et al, Description and validation of a limb scatter retrieval method for Odin/OSIRIS. Journal of Geophysical Research, 113(D04308), 2007) are successfully used."*

The first sentence in Sect. 3.2 has been removed. At the end of the section a new paragraph is added mentioning other methods.

*"Eq (5): linearization. It would be good to add some discussion about the noise here. Can the noise be linearized also without disturbing its "normality" distribution. The algorithms seem to assume normally distributed noise. The should be commented."*

Actually, there is no reason to concern about the normality of noise after the linearization because in the reality it is not normal even before. Moreover, the optimal estimation retrieval assumes that there is only a statistical noise which is also never the case. Unfortunately, we (all retrieval teams) have to accept that we use the statistical methods outside their applicability (at least from a pure mathematical point of view). As a results our estimations might be not perfectly "optimal" but this is the best one can do.

*"Eq(8): The difference btw  $F$  and  $\hat{F}$  is not clear (not clear if similar to  $\hat{y}$  which refers to normalized spectra)"*

It is similar. An explanation is added in the text.

*"Eq(11) please, explain notation  $\{ \}$  here."*

The notation is explained.

*"P5091 – L19. The sentence is not clear: In conclusion, ..."*

The sentence is rewritten.

*"eq(18). Should it be  $I_0$  (not  $\mathcal{I}_l$ )?"*

No, this is a notation for the  $I_0$  correction. The letter  $I$  is used to denote the intensity.

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"Eq(29) and P5101 – L3. Covariance matrix  $S_\epsilon$ . In my understanding the dimension here should be the dimension of  $c$ , ie, altitudes and not wavelengths. What does it mean spectrally uncorrelated in his context?"

No, what you mean is the a priori covariance matrix  $S_a$ . The noise covariance matrix  $S_\epsilon$  is defined in the measurement domain. Spectrally uncorrelated means that the noise at a particular wavelength does not depend on the noise levels at neighboring wavelengths.

"Eq(3), need to clarify  $p_k$  and  $s_m$  here."

You certainly mean Eq. 30. The meaning of variables has been clarified.

"P5103 - L23 ill-posed. If this concept is used it is important to clarify what is meant here: no unique solution exists? The sentence is not clear."

This is always the same for all remote sensing problems, the solution is non-unique.

"P5103 – 24: stochastic constraint – should it be statistical constraint?"

Changed to "statistical"

"P5105 - L7: What is the reason for using correlation length 1.5 km?"

The reason is to ensure the correlation of the results between the measurements tangent heights (SCIAMACHY has about 3 km vertical sampling).

"P5108 - L24: Optimal regularization parameter  $\lambda_a$ . It is unclear to me if it is assumed that there exists a optimal  $\lambda_a$  that would satisfy the needed regularization at all altitudes taking into account that the BrO concentrations change a lot depending on the altitude. (also in Bremen algorithm it varies depending on the altitude)."

Yes,  $\lambda_a$  is assumed to be altitude independent. Please bear in the mind that the relative changes in the concentrations are retrieved rather than concentrations themselves.

"P5130 – L1: It might bem good to add that the influence was negligible among those

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*radiative transfer models that were studied."*

Added

*"Figs 7 and 8 - they give rather similar message. Are both needed?"*

Yes, at least to show that the effect is similar for both tropics and high latitudes. The question how the results differ for different latitudes is often asked by reviewers and readers.

*"Section 8.3 The comparison with the TRIPLE method seems to be rather poor. Has the measurements been compared with other ground based instruments – has there been observed a bias?"*

No, there are no other comparisons for the TRIPLE instrument available.

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Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5079, 2010.

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