

Interactive comment on “Validation of IFE-1.6 SCIAMACHY limb ozone profiles” by A. J. Segers et al.

A. J. Segers et al.

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Authors comment to 'Reviewer Notes' by Yasmine Calisesi

We thank Dr. Calisesi for her extensive review and comment.

The original reviewer notes have been copied into this response and are discussed point by point.

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1. General Comments

"This paper constitutes a first attempt to characterize the impact of pointing error on the IFE-1.6 SCHIAMACHY limb ozone profile retrievals. It thus provides useful information for the improvement of this first global IFE data set."

"However, the large variability of the computed optimal profile correction, i.e. a vertical shift of the order of 1-3 km (even up to 4 km at the SH mid-latitudes) to be applied to each profile, makes it difficult to draw a simple rule for the correction of these retrievals which must then be considered case by case."

The spread in the computed optimal corrections is indeed large, but to our opinion small enough to be useful for a first correction. From Figure 6 it can be concluded that an overall shift of -2 km at all latitudes is an (on average) defensible correction. This is of course not the best possible correction for individual profiles, but will give on average clearly improved results. The latter is proved by the equivalent latitude study described in section 5, where this overall shift of -2 km is applied to all IFE profiles before comparison with sondes; the remaining differences give useful insight in the quality of the IFE profiles and where these require improvement.

"Another problem with the considered correction approach is that it relies on the quality of the used comparison data. This might be ok in the facts, but does in principle not exclude the "contamination" of the retrieved profiles by external biases."

Although ozone sondes are known to have their inaccuracies especially at high altitudes, the overall quality is rather good and well known. To avoid the known inaccuracies as well as possible, the use of sonde data has been limited to altitudes below 10 hPa air pressure in this study. Persistent biases

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of unknown origin can not be avoided of course, but to our knowledge, no persistent biases in ozone sondes are known for the area of interest of our comparisons.

"Finally, the use of true averaging kernels after the next data release might change the comparison results considerably. As mentioned by the authors, the presented correction can thus only provide an insight in the IFE-1.6 SCIA retrievals biases, but won't be able to substitute an accurate pointing retrieval at the base of the retrieval process. Other problems also remain to be solved, as the existence of too strong gradient in the retrieved mid-latitude profiles."

We agree that the results may change significantly for new releases of IFE profiles. At time of preparation of the manuscript, the 1.6 set was however the best available set of ozone profiles (and for the years 2002-2003, it still is). The experience obtained with this set described in this paper have already lead to improvement of new releases, for example by including a-priori profiles and kernels in the product. For future retrieval and validation exercises, we hope that this work will illustrate the importance of including this kind of meta data in a profile product.

"My main concern about the present manuscript relates to the use of zero-padded identity AK matrixes with the AK a-priori folding equation (1). This doesn't make much sense to me, as in the present configuration equation (1) literally doesn't imply any smoothing at all. To my opinion it would thus be simpler to remove any reference to this equation in the present manuscript, and describe the composed comparison profiles in a more straightforward way (see also specific comments below)."

There are two important aspects in convolution with an AK: a) excluding of those altitudes from a comparison where the retrieval is not sensitive, and

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b) smoothing of high resolution profiles. Although the 'zero-padded identity AK' does not contribute to the smoothing part b), the sensitivity part a) is described in a simple and natural way. Convolved ozone sondes become automatically equal to the retrieval product at low and high altitudes, such that the IFE profiles will not be 'blamed' for being different from sondes. We agree that the same results could have been obtained in a more 'direct' way too, but this would certainly lead to a loss of generality for the description of the algorithm.

"Also, the conciseness of the text should be improved on a few occasions (see below). Despite these criticisms, I would like to acknowledge the efforts invested by the authors in this necessary study."

2. Specific Comments

- p. 4848, l.2-15: Since the authors make use of the folding formula (1), I believe it would be useful to add here a few words about the IFE inverse model (OEM, other?).

*The IFE algorithm is based on the Optimal Estimation Method (OEM).
A remark on this and reference to the book of Rodgers has been added.*

- p. 4848, l.8-11: "not sensitive to O₃ < 7 km" and "insensitive for O₃ < 12-14 km": there must be a subtlety in there that I do not understand... maybe the authors should introduce a distinction between numerical grid range and sensitive retrieval range, or mention information content or something similar.

*The first sensitivity mentioned is a feature of the retrieval algorithm,
and should be interpreted as 'the algorithm does not provide ozone*

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concentrations different from the a-priori below 7 km'. The text has been changed according to this interpretation.

- p. 4848, l. 20: "discretized to the retrieval heights": this sentence is confusing. Are the authors describing the effect of the AKs, or just explaining how the low-res. vectors are constructed from high-res ones? In the first case, the word "discretized" should be avoided. In the second case, I would propose something more like: "the state vector is converted from a high to a low resolution numerical grid using (weighted?) averages within the low resolution layers". If "surrounding layer" is correct, then the averaging rule should be better described. Note that the true profile is always a continuous function, so I would rather refer to a "discrete representation of the true profile" in this context.

This sentence was indeed not clear. The second interpretation is the proposed one; the sentence has been rewritten following the suggestions.

- (*) p. 4848, l. 18 and p. 4849, l. 3: Equation (1) is not really useful in the present study, as the assumed "zero-padded identity AKs" not only imply that $y_r = y_a$ at low and high altitudes, but also $y_r = y$ elsewhere. There is thus absolutely no "smoothing" implied here!!! Presenting this relationship without making real use of it in the present study seems indeed like an unnecessary complication to me. I do understand the author's problem in the absence of actual averaging kernels, but then one should either abandon the folding idea, or delay its application until the kernels become available. One alternative solution could be the use of a sample AK matrix, in order to get a flavor of the obtained profile smoothing. I would thus suggest to remove any reference to (1) in the present study, or use the latter solution. In line with this, I would also suggest to avoid the use of the word "smoothed" in connection with the sondes profiles in section 4 and 5.

The effect of the "zero-padded identity AKs" is indeed as described

by the referee. However, we think that it is still useful to use the described setup, since the convolution with a-priori profiles and simulated AK provides $y_r=y_a$ at low and high altitudes. The description of in-sensitivities of the retrieval is a very important aspect of an AK !!! The in-sensitivities could have been implemented without AKs too, but we do not think that a description of this would be more clear than the description using AKs as it is now.

The use of the word "smoothed" in the remaining text is indeed suggestive however; it has been replaced by "convolved".

- p. 4849, l.7-9: Is this sentence really needed? The present sentence is confusing, as the observing system indeed still implies some smoothing of the retrieved profile, even if the assumed AKs are identity matrixes!!! I would thus suggest to reserve the word "smoothing" to the effect of the folding with the AKs and a-priori, rather than to averaging effects. Also, what is the optional averaging we are talking about?

This is indeed confusing; the sentence has been removed. The word "smoothing" has been reserved to describing the effect of proper AKs if they would have been available.

- p. 4849, l.19-20: It is indeed pretty safe to assume that the SBUV climatology is "more or less reliable" :) !!! I believe a reference to a SBUV validation study would be useful here, so that the authors can safely assume that the SBUV climatology is reliable. Or if not, they could indicate why and where it isn't.

The somewhat sloppy description of the position of the ozone layer in the SBUV climatology has been removed; an extra reference to the technical report describing and validating the climatology has been added.

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- p. 4849, l.23-27 and p. 4850, l. 1-3: Does this longitudinal variation imply that the SBUV climatology is not representative for polar vortex conditions? Wouldn't this be a problem for the offset calculation latter on? How would for instance the results look like for, say, December 2002?

The SBUV climatology is a zonal average (geographical latitude, altitude) and thus constant in longitude. Since the south-polar vortex is not centered around the geographical south pole, a climatology based on equivalent latitude would be better. The relative large in-accuracy in the climatology is visible in the offset calculations between retrieved and a-priori profiles by a larger spread in the values under polar vortex conditions (sep-dec 2002). However, the climatology used is still useful in the retrieval, since a larger uncertainty is assigned to it under polar vortex conditions. For the offset calculations between retrieved and sonde profiles it is therefore no problem that the climatology could be more accurate.

- p. 4850, l.23-27: This is "more or less" in contradiction with the assumption made on page 4849, l. 19-20. See the above comment.

Some clarification is required and added to this line. The statement on page 4849 is only a qualitative conclusion about the direction of a pointing error. The final remark on page 4850 now includes a remark that the biases found are dependent on the quality of the climatology, but will be approved by the comparisons with sondes in the next sections.

- p. 4851, l.23-26 and p. 4852, l.1-5: see comment (*) above. It would be to my opinion simpler to renounce to the AK folding description here, as no folding indeed occurs with the assumed AKs. But with some actual AK matrix, step 1) used in combination with equation (1) would imply that no smoothing contributions would be provided by the upper atmosphere levels, where the sondes

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profiles are set to the a-priori profile. A mean to avoid this problem would be to extend the sonde profile with the IFE retrievals, so that the higher levels contribution to the smoothed profiles is on average the same as for the IFE retrievals (this should be fulfilled if the a-priori statistics is unbiased with respect to the true atmosphere).

The extension of the sondes to the top of the atmosphere is required to be able to apply convolution (1). Since no conclusions should be drawn (and have not been drawn!) about the error in the IFE profiles at altitudes where the sondes has been extended, the actual values used for the extension do not really matter. A sufficient requirement is that they are independent of the IFE profiles and look like an ozone profile, and these requirements are met by the a-priori profiles.

- p. 4853, I.1-2: how do these results evolve with the season? is there a clear influence or not? if yes, it would be useful to attribute different colors to each month results in Figure 6.

A clear monthly dependency could not be detected in the limited data available for figure 6. A larger data set was however available from the comparison based on equivalent latitude criteria, and for this set, conclusions have been drawn on the monthly evolution of the errors (figure 10; p. 4856, I.21-26).

- p. 4853, I.11: I would say that the two results are indeed images of the same thing, but not the cause of each other. The causality relationship doesn't seem appropriate to me here.

The similar biases in the tropical upper stratosphere are caused by the same thing, since they are located on an altitude where the sondes have been extended to the top of the atmosphere with the a-priori

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profiles (these altitudes are above the dotted line, as explained in the caption of figure 5).

- p. 4853, l.20-21: if possible, the use of equ.(1) with one sample realistic AK could give a more conclusive insight into this question. See also point (*) above.

Intuitively, an AK 'out of the book' would smooth a sonde profile and create less stronger gradients, making the observed bias even larger! In reality, AKs are often somewhat different than subscribed by theory, for example with larger values in the upper than in the lower diagonal parts. Only a true AK would give the required insight in this case.

3. Technical Corrections

- p. 4849, l.21: write "This displacement of the ozone layer altitude..." instead of "ozone layer..."

Ok.

- p. 4851, l.21 and several other places: replace "co-locating" by "co-located"

Ok.

- p. 4853, l.13, and several other places: remove "smoothed" here (see point (*) above).

Ok; replaced by "convolved" or used to explain the result of proper AKs.

- p. 4855, l.10 and p. 4855, l.16-19: these two sentences are confusing. Line 10: replace by "...this corresponds to a virtual meridional distance of 250 km..." or anything similar. Line 16-19: replace by "if the horizontal distance corresponding to the equivalent latitude criterion is larger than 1000 km..."

Ok.

- p. 4856, l.20: write "for December at the mid-latitudes".

Ok.

- p. 4857, l.16: write "according to the results of the comparisons with the a-priori..."

Ok.

- p. 4857, l.27 and p. 4858, l.1: application of the actual kernels will influence the comparison results through a modification of the degraded sondes profiles, but it won't modify the retrieved gradients. Write: "...impact on the comparison results".

Ok.

- p. 4864, legend: write "Bias (left) and ... (right)".

Ok; latexed locally, the plots occurred on top of each other.

- p. 4866: a) and b) are indicated somewhere in the manuscript but are not mentioned on the figure.

Ok; the 'a' in the text should have been removed.

- p. 4869: provide an "altitude" label for the y-axis, maybe on the right-hand side.

The old labels were indeed not clear; the left axis has now an 'altitude' label, while the month is put as text in the upper right corner of each axis.

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