

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

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Received and published: 8 August 2005

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 midlatitudes) 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

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case by case. 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. 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 midlatitude profiles.

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). 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?).

p. 4848, l.8-11: "not sensitive to $O_3 < 7$ km" and "insensitive for $O_3 < 12-14$ 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.

p. 4848, l. 20: "discretized to the retrieval heights": this sentence is confusing. Are the authors describing the effect of the AK's, 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.

(*) 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 flavour 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.

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 AK's are identity matrixes!!! I would thus suggest to reserve the word "smoothing" to the effect of the folding with the AK's and a-priori, rather than to averaging effects. Also, what is the optional averaging we are talking about?

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

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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.

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 later on? How would for instance the results look like for, say, December 2002?

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.

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 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).

p. 4853, l.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.

p. 4853, l.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.

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.

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3. Technical Corrections

- p. 4849, l.21: write "This displacement of the ozone layer altitude..." instead of "ozone layer..."
- p. 4851, l.21 and several other places: replace "co-locating" by "co-located"
- p. 4853, l.13, and several other places: remove "smoothed" here (see point (*) above).
- 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..."
- p. 4856, l.20: write "for December at the midlatitudes".
- p. 4857, l.16: write "according to the results of the comparisons with the a-priori..."
- 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".
- p. 4864, legend: write "Bias (left) and ... (right)".
- p. 4866: a) and b) are indicated somewhere in the manuscript but are not mentioned on the figure.
- p. 4869: provide an "altitude" label for the y-axis, maybe on the right-hand side.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4845, 2005.

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