Atmos. Meas. Tech. Discuss., 6, C2337–C2345, 2013
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Atmospheric Measurement Techniques Discussions

AMTD

6, C2337–C2345, 2013

Interactive Comment

Interactive comment on "Error budget analysis of SCIAMACHY limb ozone profile retrievals using the SCIATRAN model" *by* N. Rahpoe et al.

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Received and published: 2 September 2013

Bold: **Referee's Comments,** Normal font: Author's Reply

Anonymous Referee 1 Received and published: 12 July 2013

This manuscript provides an error budget analysis for SCIAMACHY limb ozone profiles based on a propagation of error methodology that uses parameter error estimates and related changes in a radiance forward model, followed by resulting changes in the retrieved ozone profiles. Overall, in terms of the scientific significance, quality, and presentation, within the bounds of AMT, I rank this

work as Good or slightly higher, as it currently stands. The information that is provided and the conclusions are fairly robust (with some possible exceptions, as some total error estimates may really be lower limits ::: see below). This work will help data users in their understanding of ozone profile uncertainties for the SCIAMACHY retrievals. The methodology is presented in a clear enough way and the overall analysis is straightforward enough to follow. The presentation, discussion, and Figures are generally clear and supportive of the conclusions, except as noted below. I support publication of this manuscript after some consideration is given to a few spe- cific issues mentioned below, mainly in terms of other possible error sources. A few more minor and editorial-type comments are also provided.

Specific Issues While the manuscript's analyses seem solid to me, the authors should give more spe- cific attention (and references) to past work, including for this particular type of mea- surement, and for other general references regarding the overall methodology. On the overall methodology, for example, other studies have used sensitivity analyses in a similar way to study/provide uncertainty estimates from both systematic and ran- dom sources. For example, the manuscript mentions one of the most complete studies regarding limb scattering ozone algorithm sensitivity (by Loughman et al.); see also Rault and Taha [JGR, 2007]. More discussion in relation to that work should be pro- vided (see below). Some other references are also provided in the manuscript (e.g., for MIPAS work). Other studies that have used this general methodology in the past (and that could be mentioned) include the (ozone-related) error analyses for the Microwave Limb Sounder on the Aura satellite [e.g., Livesey et al., JGR, 2007; Froidevaux et al., JGR, 2008]; the latter studies include a full day of sensitivity tests for various param- eters. Regarding the study by Loughman et al., since that one applies to the same sort of measurement technique, I feel that more discussion should be provided to give read- ers some feeling for the consistency

versus those results. The main error sources (tangent height and stratospheric aerosol knowledge) are discussed in this manuscript and the Loughman et al. work, and agree in the results to first-order at least, but this is worth commenting about to some extent. Cloud effects are also considered in the manuscript (but not in the Loughman work). However, instrumental error sources (or even things like imperfect knowledge of the angles for the instrument positioning on the spacecraft) are completely ignored in the current manuscript, but one wonders why or what possible error impact is being neglected regarding such potential issues. Table 4 of the Loughman publication gives a fairly comprehensive range of other potential error sources. According to that reference, nitrogen dioxide (imperfect) knowledge, a priori information, and wavelength mis-registration could add several percent additional error depending on the height; stray light and polarization effects are other issues dis- cussed by these authors, although not considered to contribute in a large way to the total errors. Even if no detailed study is repeated for this manuscript, at least a mention of such possible effects using Loughman et al. as a reference would be a more thorough represen- tation of the fact that the manuscript's current total error budget is likely to be somewhat underestimated, even if it is only by a few percent or less in some regions/altitudes. An- other apparent discrepancy between these results is that the tangent height sensitivity is smaller in this manuscript, as a result of the use of better knowledge, but this can be mentioned also, as long as it is robust enough knowledge. The work by Boccara et al. [JGR, 2008] regarding the accuracy of ECMWF (and NCEP) analyses is worth men- tioning as well. This seems to support the temperature uncertainty impact provided in the manuscript (and the use of a 1-2K temperature error estimate).

We have added a Discussion section (See section 5) in the paper. Most of references mentioned by the reviewer have been used now to compare our results with previous works.

AMTD

6, C2337–C2345, 2013

Maybe the authors could mention why they chose to perform somewhat more limited tests on a fairly small number of profiles and conditions (maybe this relates to the computer time involved to do more comprehensive tests), although it does seem like the tested range is probably large or typical enough.

Measurements on 5 different days - covering three seasons - are used now. In order to increase the number of profiles 9 additional orbits of the year 2008 are added, that increases the total number of profiles for the calculation up to 204 in the northern hemisphere and 137 profiles in the southern hemisphere.

Also, in the Conclusions, on line 15 (page 13), you refer to 4% errors from tangent height, clouds, and pressure, whereas tangent height can produce 7 or 8% errors (max) according to Figs. 5 and 7; clouds also can lead to > 5% errors at least in the tropics ::: Is it not also true that clouds will completely negate retrievals at some point, or below them? More mention of this could/should be made. The 4% value does not seem to be the right number (for tropics) and it is not clear if it refers to each component or the sum of these, the way you worded this. Please clarify this for the conclusions (this should also agree with the Abstract, which seems more accurate).

In order to be more precise, the sentence is now changed into: *'The ozone retrieval errors associated with errors in tangent height, clouds and pressure dominate the total systematic error for altitudes above 20 km and can lead to total systematic error on the order of ±5% at these altitudes.'*.

- Page 2, line 12 (Abstract): I suggest changing “most part” to “most”.

Corrected

- Page 3, line 8: Is Version 2.3 the version that was studied for the sensitivity analy- ses, and for which the error budget applies? Maybe this should also be stated in the Abstract, even if future versions do not necessarily imply significant changes to error

Added

- Page 3, line 18: "sum" here really should be interpreted as geometric sum, or root sum square, in terms of most common use of the combination of such errors, although there is no rigorous way to best do this for non-random errors (indeed, a simple sum can be a more conservative approach). Maybe just stating that total error "arises from a combination of accuracy and precision estimates of the ozone profiles" would be sufficient, rather than "sum"; indeed, the reference mentioned (Cortesi et al.) does not use a simple arithmetic sum.

Corrected

- Page 5,line 5; typo on the word "regularization".

Corrected

- Page 9, lines 7/8: A decrease of 1

AMTD
6, C2337–C2345, 2013

Corrected

- Page 9, line 10, change "in the order of" to "of the order of". Also, line 11, delete "part", and line 15, change "that" to "when".

Corrected

- Page 12, line 10: typo in word "distinction".

Corrected

- Page 13, line 2, please specify what is meant by tropics here (20S-20N, 30S-30N, or?).

30N-30S—now corrected

- Page 13, line 22/23, I would state "for altitudes above 15 km" rather than "for the altitude range of z $>$ 15 km". Similarly, on line 26, one could say "for altitudes below 20 km".

Corrected

- Page 14, line 6, I see no need for colons after "systematic" and "random".

Corrected

AMTD
6, C2337–C2345, 2013

Interactive Comment

- The sentence on lines 7/8 does not add anything useful and should be deleted or clarified (how does one really try to study "unknown parameters"?). However, I did mention a few known parameters that others have studied in the past, and that were neglected in this study.

The sentence has been deleted, and one with underestimation of the total error added (See conclusion).

- Tables 2, 3, and 4: I personally find that there is almost too much information in these Tables, and the reading would probably be clear enough regarding main results if fewer significant digits were used (as they are often probably not significant or too small to have an impact, e.g., anything under 0.1 could be listed as 0.1). Numbers like 29.8 and 33.7 can easily be written as 30 and 34 and things then become easier to read (and even understand). Even if the authors keep more detailed Tables for their own information, most readers are probably satisfied with the knowledge that 1.66 or 1.55 for pressure impact in Table 2 at 20 km is about 1.6There are just a lot of numbers that a reader could try to digest, but many of the numbers are not adding information, after the reader actually reads through them all. Table 5 is the main result as a good summary and fewer digits; the other Tables could be in supplementary material, or somewhat shortened for easier reading - this is just a suggestion.

We now use fewer digits, and will add the tables in the supplementary section.

Also, in Table 2, do you want/need a sign associated with the uncertainty that was applied to each parameter? I would find it clearer to state that pressure was

AMTD
6, C2337–C2345, 2013

changed by + or – 1

Only one sign of the calculation is shown in the tables, therefore it is important to mention, which one has been used.

- Figure 1: Adding a zero line for horizontal would be helpful to guide the eye. Also, is the "Res" value an average over the time period? It would be better to provide an rms value (or both) – as it is not very informative to know that the average residual is close to zero.

We added a zero line.

- I find the notation in Figures 2 and 3 for the Ref somewhat confusing; for example, for pressure error, was a reference chosen as a 1.01 deviation (and you also plot 1.00 and 1.02 for sensitivity testing)? The tangent height error means that you used 200 meters as a reference run, and then applied 0 meters and 400 meters, rather than using -200 and + 200 meters as sensitivity tests? For T-ozone, things look asymmetric, as you list 1.02, 1.00, and 1.03 Please clarify.

We accept your crititque, now we have used the $\pm$ signs to be more specific (See the legend). Instead of 0 m and 400 m relative to the reference of 200 m, we now show the reference of 200 m with $\pm$ 200 m cases. For T-ozone the data were available only with these configuration and unfortunately with no other settings. At least we can see that asymmetry is linear.

- In Figure 4 (legend), please clarify that only one sign of the change was used

AMTD

6, C2337–C2345, 2013

Interactive Comment

(as many of the changes seem to be either positive or negative).

Now we added the correct sign for the figures (See the legend).

- In Figure 8, why do you need to show the positive and negative sides if these are symmetric curves (are they symmetric – hard to tell)? If they are symmetric, one could see the changes somewhat better by "blowing up" just the right side of this sort of plot, and just stating that these are absolute (or rms) error estimates.

This is correct, now we show only the 'one' side of the plot.

We would like to thank Anonymous Refree for the extensive and detailed review of the paper. The critique and suggestion made herein has improved the whole structure of the paper. We hope that we were able to cover most part of the suggestions.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/6/C2337/2013/amtd-6-C2337-2013-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4645, 2013.

AMTD
6, C2337–C2345, 2013
Interactive Comment