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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #1

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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 specific 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 specific attention (and references) to past work, including for this particular type of measurement, 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 random 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 provided (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 parameters. 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.

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 readers 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 discussed 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 representation 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. Another 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 mentioning as well. This seems to support the temperature uncertainty impact provided in the manuscript (and the use of a 1-2K temperature error estimate).

Also, in the Conclusions, on line 15 (page 13), you refer to 4

Minor and editorial comments/suggestions

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

- Page 3, line 8: Is Version 2.3 the version that was studied for the sensitivity analyses, 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

analyses.

- 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. . .

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

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

- 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".

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

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

- 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".

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

- 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.

- 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.

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 changed by + or – 1

- 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.

- 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.

- In Figure 4 (legend), please clarify that only one sign of the change was used (as many of the changes seem to be either positive or negative).

- 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.

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