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Atmospheric Measurement Techniques Discussions

Interactive comment on "Nadir ozone profile retrieval from SCIAMACHY and its application to the Antarctic ozone hole in the period 2003–2011" *by* Sweta Shah et al.

Sweta Shah et al.

moonkostar@gmail.com

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Response to Anonymous Referee #3

We thank the referee for the constructive comments and address them point-by-point. The comment is copied below in normal font, and the response is in italics.

The paper has been re-organized in order to make the focus on SCIAMACHY L1 evaluation clearer: the part on the Antarctic ozone hole has been removed from the paper, and the Appendix has been incorporated into the main text. A new figure showing profiles of the DFS for all seasons and two representative years has been included.

The text has been revised throughout the paper. The textual changes are indicated in colored text in the manuscript version showing differences.

General comments

1. The focus of the paper should be narrowed down. If the focus of the study is on evaluation of different Level1 data, than the part with the Antarctic ozone hole should be removed from the paper, the title needs to reflect the goal of the study, and appropriate changes should be made in the abstract and conclusions. In Introduction (page 2) authors presented a very "skewed" overview of the existing satellite methods of retrieving ozone profiles. Ozone profile retrievals derived from nadir SBUV/SBUV-2 sensors span for more than 40 years. There is also a number of limb ozone profile datasets, besides described SCHIAMACHY limb, with high vertical resolution, like MLS, MIPAS, OSIRIS and OMPS-LP, as well as from occultation instruments like ACE-FTS, GOMOS and SAGE II. Some of these datasets overlap with SCIAMACHY mission and can be used for validation in addition to sonde data.

   Author Response:
   As requested by the reviewer we have narrowed down the focus of the article to an evaluation of different L1 datasets. The ozone hole part has been removed. The title has been changed accordingly. In the Introduction we added other types of satellite ozone profile measurements, but we do not aim for completeness, since our focus is SCIAMACHY.

2. Section 3: I do not understand what had been done with the Slit Function in this study. Have you applied the SF corrections described in section 3 to Level 1 data before doing inversion? Did you apply these SF corrections to all 3 versions of Level 1 data? Or do you provide description of the SF corrections that had

already been implemented in Level 1 data? Please, clarify that in the text.

Author Response:
We agree that this was not clear. We used the default slit function in the retrievals. We evaluated the slit function of SCIAMACHY to see if we can calibrate the solar spectra better. The answer turned out to be yes, however the calibration errors from insufficient degradation corrections are significantly worse than those from the slit function. Thus applying slit function fitting would not significantly change the results. This is now presented in a more organised way in the Discussion section.

3. Figures 6 and 7 show differences with sondes for early and later years of SCIAMACHY mission. It is obvious from these figures that the vertical pattern of differences has changed significantly (not just the absolute differences) over the instrument lifetime. It is not reflected in the discussion, however it is very important for any scientific application as it will lead to wrong conclusions. Obviously, such a change in the vertical pattern of differences points to a significant drift in O3 retrievals. Thus, you can not draw any reliable conclusions about inter-annual ozone variability over Antarctica using these O3 dataset. If the goal of the paper to look on effect of Level 1 adjustments, then it is worth to look at O3 time series at several levels to determine time-dependent changes and connect them to a drift in different spectral channels.

   Author Response:
   We thank the reviewer for this important observation. Since we have omitted the application to the Antarctic ozone hole, this comment is not so relevant anymore for the current paper. We added this remark to the discussion of the figure with ozone profile validation for all years (now Fig. 7).

4. Sections 4-6: All captions for figures and tables should be revisited and all data/lines should be clearly explained (see more below in "specific comments").

   Many conclusions presented in these sections are not obvious for readers and more explanations and evidences are needed.

   Author Response:
   We have revised all captions of figures and tables. The text of the paper has been revised to improve clarity.

5. Section 7 seems to be disconnected with the previous discussion. It is not clear what is the purpose of this section.

   Author Response:
   The discussion section is revised. It now contains the calibration issues for potential future improvement of the L1 data, which connects it with the rest of the paper which presents the evaluation of the current L1 datasets.

6. It was mentioned several times in the text and in the abstract that the focus will be on the stratospheric ozone. And this would be a reasonable approach, since nadir sensors are not expected to produce high quality ozone profiles in the troposphere (especially relative to sonde measurements). It is not clear to me why results below 200-300 hPa are shown in the figures. Also several tables have extra lines to show statistics for the tropospheric values. It concerns me that some of the conclusions regarding to performances of different versions are based on the tropospheric results rather than stratospheric.

   Author Response:
   We prefer to show the entire retrieved ozone profile, thus including the troposphere. However, we focus on stratospheric ozone, since the validation shows that the retrieval has a poor performance in the troposphere. Our conclusion is that the retrieval is most reliable in the stratosphere and we mention this more clearly in the paper in the conclusions and validation sections.

Specific comments

1. Section 2.2, page 6, lines 13-15: Since the vertical resolution of nadir measurements are limited they are sensitive to the a priori profiles as well as to assumed a priori and measurement covariance matrix. Please, specify which a priori data were used in the study, because Table 2 says that 3 different a priori data sets available in OPERA. Also, please, explain how matrices required for the Optimal Estimation were set in your study. Do Level 1 SHIAMACHY data come with the uncertainties that you use in the retrieval algorithm or do you have to assume these uncertainties? Are these measurement uncertainties the same for all Level 1 data sets that you tested here?

   Author Response:
   We used the McPeters et al., 2007 a-priori profiles; this is now mentioned in the text. We have used the measured reflectance variances and a relative noise-floor (systematic noise) of 0.15.

2. Figure 4. Please, specify in the caption what horizontal dashed lines represent. Are they shown here to make a connection between specific spectral ranges in radiances to altitude range in O3 profiles? It's also would be helpful to add the mean/median equatorial sonde profiles for 2003 and 2009 as a reference.

   Author Response:
   The horizontal lines are explained in the text. Note that this is Figure 3 in the revised paper. We prefer to keep the comparison with sondes to Sec. 4, because there we compare the sondes systematically with the SCIAMACHY profiles.

3. Sections 4-6: Median values for biases and other characteristics are reported instead of mean values. Do you have a specific reason for using median values? Have you found many outliers or do you believe that the distribution of differences

   is mostly skewed to a particular direction, so the mean values are not representative? Please, explain that in the text.

   Author Response:
   We found a few outliers, but not many. However we choose medians because we consider them more representative than the means when using large datasets, which is the case here.

4. Section 5.2, lines 1-10: This part of the text is very confusing. Do you make a conclusion "This suggests that the quality of nadir L1 data is still poor" based on the fact that differences in tropospheric ozone are too large? This part needs to be revised.

   Author Response:
   Agreed. We revised the text.

5. Appendix. The Averaging Kernels for nadir observations change significantly with latitude and season. Showing just one example of the AK is not sufficient. I would suggest to show DFS profiles for different latitude bands and seasons, and specifically for the Antarctic latitudes in Sep-Oct. I also believe that this discussion belongs to section 2.2 where you should describe the main characteristics of the SCHIAMACHY nadir ozone retrievals.

   Author Response:
   Following the suggestion of the reviewer, we added a new figure, Fig. 4, showing DFS profiles for different seasons for the years 2003 and 2009. See also response 6 to Referee #1.

6. Section 5.1: On my opinion authors didn't provide enough evidences and explanations to demonstrate that version v8 is any better than v7_mfac.

   Author Response:
   Indeed the differences between version 8 and version 7 with m-factors is not

large. However, there is a small improvement in using version 8, as can be seen from the standard deviations in Table 3 and the percentage differences in Table 4.

7. Section 5.2: I believe that results presented in Fig. 7 and Table 5 are not enough to call this section 'Validation of v8'. For instance in section 7, authors speculate that on the days when the instrument was heated the measured radiances were affected. Have you tried to isolate and remove those days from your analysis? Do you see improvements/changes in the results?

   Author Response:
   Our focus in the paper was to evaluate the different existing versions of L1 data. The referee makes a valid point of analysing and experimenting in detail with the radiometric and bias corrections. However this is beyond the scope of this paper. We reserve this to a future study. We limit ourselves in Sec. 5 to a discussion of potential future L1 improvements which should lead to better ozone profiles.

8. Section 6: The analysis shown in this section is insufficient. There are many total ozone observations available for the considered time period that can be used to validate integrated ozone columns instead of looking at the reanalysis data. Also, the statement in the conclusion "we investigated the Antarctic ozone profile behavior in the austral spring season" doesn't correspond to the work shown in Section 6. There are many satellite ozone observations that overlap (or partially overlap) with SCHIAMACHY mission like SBUV/2 NOAA-17, Odin OSIRIS, ACE-FTS, Aura MLS and MIPAS. Comparisons with these correlative measurements would help you to understand how well SCHIAMACHY nadir profiles can describe the vertical ozone distribution in- side the ozone hole, and therefore if this dataset is suitable for studying inter-annual ozone variability over Antarctica. Without this extensive analysis it is not possible to claim that SCHIAMACHY nadir ozone profiles can be applied for the scientific analysis.

   Author Response:
   Since we omitted the Antarctic ozone analysis in the revised paper, this comment is not applicable anymore.

Minor correction/typos:

1. Section 6 p 14 line 35 and page 15 lines 1-2: I don't see the cost function or # of iterations (that are not shown in Table 6!!!). Do you mean Table 7 here?

   Author Response:
   We omitted the Antarctic ozone analysis, so this is not relevant anymore.

2. page 5, line 29: Should be "This allows" instead of "This amounts"

   Author Response:
   Corrected.

3. Table 2, Pressure grid- it would be useful to see the pressure grid used in your retrieval algorithm

   Author Response:
   The retrieval pressure grid is given in Table 2.

4. Table 3. Please, in the Table caption make a connection to the corresponding Figure 4. Also, it's not clear what do you mean by # of pixels? Is it a number of profiles considered for this comparison? Why is it different for different versions of Level 1 data? Please, spell "n_inter" as "number of iterations" in the caption. Could you explain what does it mean "median n_iter for #pixels"? Do you mean median number of iterations for the considered pixels (Profiles)? It should be "Column 7: standard deviation of Column 6".

Author Response:
We revised Table 3 and its caption according to the suggestions of the reviewer.

5. Tables 4 and 5. Please, fix "Troposphere [1000-100] hPa" and "Stratosphere [100- 10] hPa". The values shown in columns 6 and 7: are these biases for integrated stratospheric/tropospheric columns or mean over specified altitude range?

   Author Response:
   We corrected the "Troposphere [1000-100] hPa" and "Stratosphere [100-10] hPa". The values in columns 6 and 7 are medians over the specified altitude range.

6. Table 6: Please, clarify what quantities are shown in columns 2-4. Do they show min and max numbers of O3 profiles used to calculate the daily zonal mean value? It says "Column 5: Median uncertainty in stratospheric ozone column". Is it a correct label? What did you use to determine the tropopause pressures? Also why did you show total ozone values on Figure 8 and not stratospheric columns?

   Author Response:
   Since we omitted the Antarctic ozone analysis, this comment is not applicable anymore.

7. Table 7. Please, explain what is "relative uncertainties in ozone layer per height". Section 6, Figure 8 and Table 6 show results for total ozone columns. The numbers for sigma shown in Tables 6 and 7 are not the same. Please, add clear explanation of results shown in these tables.

   Author Response:
   See response above.