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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tropospheric column amount of ozone retrieved from SCIAMACHY limb-nadir-matching observations" *by* F. Ebojie et al.

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Author comment on "Tropospheric column amount of ozone retrieved from SCIAMACHY limb-nadir-matching observations" by F. Ebojie et al. doi:10.5194/amtd-6-7811-2013.

Response to anonymous reviewer #1 (C2303)

We thank the anonymous referee #1 for his/her valuable comments, most of which we agree with. We believe that this review has helped us to improve our manuscript.

Reviewer #1 (Comments to Author):

This paper is a study for determination and validation of tropospheric column ozone by using the difference between total ozone from SCIAMACHY viewing geometry and stratospheric column ozone from limb viewing geometry for long period of 2003 - 2011. Even though there have been many studies about residual based tropospheric ozone derivation and validation, this kind of study is worth of publication. However, this paper still needs to find a better way of description about significance of this study relative to previous studies as well as results.

Reply: We thank the reviewer for his/her encouraging comments. We have added in sections 1 and 3.3 of the revised manuscript a discussion about the significance of this study relative to previous studies as well as the results.

Some statements such as the following are included in the revised manuscript:

"The global retrieval of tropospheric ozone from SCIAMACHY using the limb-nadir-matching (LNM) technique is useful, because it does not require any further assumptions, such as zonal homogeneity of stratospheric ozone or an estimate from model data. The algorithm extracts tropospheric ozone information from similar air masses probed in both nadir and limb geometry."

"The unique approach of the SCIAMACHY LNM is that column (nadir) and profile (limb) data are provided by the same instrument thus providing a dense sampling of tropospheric ozone on a global scale. There is no calibration issues as it is, in methods that involve two different satellite measurements."

General comments: 1. The residual ozone does not use simultaneous measurements of total and stratospheric ozone. Therefore, it is expected to have error when and where significant short time variation of ozone occurs. Therefore, the error should be dependent on season and location. However, authors keep insisting that SCIAMACHY-residual tropospheric ozone column is "good agreement" or "similar pattern" with ozonesonde and other residual tropospheric ozone. However, when I look at the figures from 6 to 13, some of them agree but some of them are not. So, the authors

should discuss the disagreement as well as agreement.

Reply: Thank you for this point. The combined limb-nadir measurement mode of SCIAMACHY enabled the instrument to observe the same atmospheric volume first in limb and then after about 7 minutes in nadir geometry.

We have included the following information in section 5.1 of the revised manuscript: "The error, which could have resulted from the time span (approximately 7 minutes) between limb and nadir observations for SCIAMACHY as well as for MLS and OMI due to dynamics, is generally insignificant because the time mismatch between both observations is relatively short and the horizontal resolution of the limb measurement is rather coarse."

We have also discussed the agreement and disagreement in the comparison plots. We have replaced figures 6 – 13 in the original manuscript with figures 6 – 11 in the revised manuscript.

The following sentences are included in section 5.1 of the updated manuscript: "The difference in the tropospheric ozone columns might be due to different tropopause heights employed in the retrievals from the different instruments. Differences might also come from retrieval algorithms in terms of a-priori value, cloud treatment and air mass factors employed by the different satellite instruments. Comparison of tropospheric ozone columns from satellites with the values from ozonesonde, which are sparse, are prone to noise mostly in dynamically active subtropics where rapid fluctuation in tropopause heights occur. Such comparisons exhibit a lot of scatter and the regression line deviates from the line of unity."

2. The validation of SCIAMACHY tropospheric ozone is not easy because there is neither perfect ground truth because of limited ozonesounding measurements nor perfect satellite measurements. Therefore, the comparison should be performed not only based on absolute ozone value such as monthly averaged ozone, but also the variability and morphology of ozone. In this matter, the paper appears to focus on showing

absolute value comparisons such as Table 4 but have a lack on showing some disagreement in variability and morphology in comparison. For example, the results used for Table 4 should find a better way of expression indicating not only monthly averaged ozone but also variability.

Reply: Thank you for mentioning this, mean and standard deviation are presented now in table 3. We have discussed in section 5.1 of the updated manuscript that the standard deviation shows an aggregate of the variability of tropospheric ozone values retrieved from the different instruments.

Following the reviewers' suggestion, anomaly time series and latitudinal cross section plots from different satellite instruments have been included in the manuscript (please see figures 6-11 and 13).

3. In most of cases, OMI/MLS-tropospheric ozone is always smaller than other measurements. This could be due to either MLS stratospheric ozone is larger than SCIAMACHY stratospheric ozone, or OMI total ozone is smaller than SCIAMACHY total ozone. So, I think it is relevant to compare SCIAMACHY total ozone and stratospheric ozone with OMI total ozone and MLS stratospheric ozone because the difference of two ozone results in difference in tropospheric ozone.

Reply: Thank you for pointing this out, we have included the following information in sections 2.2 and 5.2 of the updated manuscript: "The low bias in the AURA tropospheric ozone is most probably due to MLS ozone being high biased in the lowermost stratosphere (e.g. Livesey et al., 2011)."

Zonal climatology of stratospheric ozone column, total ozone column and tropospheric ozone column are included in the revised manuscript (please see figure 13). This is discussed in section 5.1 of the updated manuscript.

We also pointed out in section 3.2 of the revised manuscript that WFDOAS total ozone from GOME and SCIAMACHY is mature and agrees with other total ozone datasets to

within 1% (e.g. Bracher et al. 2005, Weber et al., 2005, 2013).

The validation results of OMI TOC and MLS stratospheric ozone are also discussed in section 2.2 of the updated manuscript

4. The TES has very narrow viewing geometry. I believe it has only one or two data points for a month in a given location and so does not have enough data for comparison with monthly averaged SCIAMACHY tropospheric ozone shown in Figures 6 through 13. If so, the author should explain relevance of using TES data for SCIAMACHY tropospheric ozone validation.

Reply: We agree that TES might have sampling bias because of low number of measurements per month, which could lead to a bias in comparisons with other data products. Despite a possible bias, TES data product is still valuable because of a lack of tropospheric ozone data products from space borne instruments. We have discussed possible sampling issues of TES more carefully in section 2.2 of our revised manuscript.

5. In Figure 14, the highest tropospheric ozone is observed over high latitude greater than 60 degree north.. This is not very different from ground-based measurement of tropospheric ozone. The authors should explain this.

Reply: We believe that increase in tropospheric ozone amounts in the northern high latitudes is mainly due to the transport of ozone plumes from the midlatitudes to the higher latitudes (e.g., Thompson et al., 2007a, 2008, Pfister et al., 2008). The above statement is included in section 5.2 of the revised manuscript.

6. In Figure 15, it seems that the largest difference occurs in DJF period. I think this should be discussed.

Reply: Thank you for this point. We have mentioned in section 5.1 of the revised manuscript that differences are larger during DJF. Line plots on tropospheric ozone climatology, which show the variability from the different instruments have been added

in the manuscript (please see Fig. 12). All datasets show higher values in the NH extratropics during DJF.

7. This paper has too lengthy introduction. It should be reduced.

Reply: We have reduced the introduction as requested.

8. Even though this paper just used the ozonesounding data, Section 2.3 for ozonesondes is too much in detail.

Reply: We have shorten the discussion and analysis in Section 2.3 for ozonoseondes as requested.

9. In Figure 6 through 13, the latitude and longitude symbols, N or E, are missing.

Reply: We have replaced figures 6 - 13 with tropospheric ozone column anomaly [DU], see new Figures 6 - 11. The missing information in the previous plots are now included in the new plots.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 7811, 2013.