

Referee Report to “Synergy of Using Nadir and Limb Instruments for Tropospheric Ozone Monitoring” by Viktoria F. Sofieva et al.

The manuscript describes a new approach to estimate tropospheric ozone column in the framework of the residual method by using multiple data sets from limb-viewing instruments to calculate the stratospheric ozone column. Methods to homogenize and interpolate the data are described. The suggested approach is certainly of interest for the scientific community and, in general, the study is of a good quality suitable for publication in AMT. However, there are a few deficits in the study that need to be addressed before the publication. These are mainly insufficient justifications of the approaches and obtained results. My detailed comments are listed below.

Major comments

- Sect. 3: I do not fully agree with the concept of the correction of the upper tropospheric ozone column using the data from an external source used by the authors to remove the UT contribution. From the point of view of atmospheric dynamics, I'd expect that the main source of the UT ozone is its transport from the lower troposphere. If the authors have a different opinion they have to provide and justify it. If the UT ozone is determined by the tropospheric pollution, it should be closely related to the ground sources and transport processes. Thus, the described correction can only work properly if the dominating ground sources do not change their strength and location. To my opinion this method can result in artifacts if distribution of the ground sources changes. With that it is not clear what will be the goal of the corrected data. Please provide more discussion/justification in the paper.
- The superiority of the interpolation approach over the data assimilation is stated but, in my opinion, not well justified.
- Supplement 3 is meant to demonstrate a good agreement of the small-scale ozone variability in OMI and SILAM data. Looking at Figs. 8-10 I cannot follow how the authors come to the conclusion that the agreement between the modeled and experimental data is very good, e.g. I see nothing in common between black or between red curves for 60°S – 90°S in Figs. 9 and 10. As this part is not highly relevant for the rest of the study this supplement can be removed. Otherwise comparisons and justification of the conclusions must be improved.
- The provided comparisons for the tropospheric ozone are too sparse. Plots illustrating time series need also be provided (preferably as 2D plots rather than contours as the latter are much more difficult to compare). The provided comparison illustrates that SUNLIT results are somewhat different from other data but no attempt is made to investigate, which dataset should be considered as a better one. Comparisons with ozonesondes for the resulting tropospheric ozone values (preferably including results from other datasets) are clearly missing and have to be added.

Minor comments

- In Abstract time ranges of the created data sets should be mentioned
- Page 2, lines 50-52: this is only true for the along line of sight direction. The resolution can be much higher in the across direction, e.g. ALTIUS, CAIRT.
- Page 2, line 54: Presence of clouds is also a problem for the nadir measurements and for the usage of the residual method in general.
- Page 2, line 63: Please add “Leventidou, E., Eichmann, K.-U., Weber, M., and Burrows, J. P.: Tropical tropospheric ozone columns from nadir retrievals of GOME-1/ERS-2, SCIAMACHY/Envisat, and GOME-2/MetOp-A (1996-2012), Atmos. Meas. Tech., 9, 3407-3427, <https://doi.org/10.5194/amt-9-3407-2016>, 2016.” to the citations
- Page 3, line 71: The data calibration is not a serious issue then combining total/stratospheric ozone columns retrieved with DOAS-like methods
- Sect. 3.1: It is not clear why the UTLS region is treated separately, as UT is the inherent part of the troposphere and contributes to the tropospheric ozone while LS is a part of the stratosphere.
- Page 8, paragraph starting at line 200: It is incorrect to talk about UTLS here as you only consider the region below the tropopause and since do not enter the lower stratosphere (LS).
- Page 8, Sect. 3.2: I am wondering if the observed large influence of the UT region is specific to the selected method to determine the tropopause. Do the conclusions remain the same if using blended tropopause?
- Page 8, Sect. 3.2: The name of the section might be sub-optimal as one expects rather a discussion about integration effects. Here, “vertical extent” would be more appropriate.
- Page 9, line 223: The sentence duplicates information already presented in the two sentences above.
- Page 9, reference to Fig. S2: information needs to be given how the sampling for this plot was implemented, e.g. in accordance to which instrument’s sampling pattern.
- The same comment as above applies to Figure 4.
- Page 9, Figure S3: Altitude axis in km should be provided in addition.

- Page 10, second item: “the observed ground-level ozone enhancements” is an incorrect formulation. As follows from the previous discussion, the ground-level ozone enhancements are almost not seen by the instruments. The increased ozone amounts become detectable when air masses raise over the boundary layer.
- Page 10, third item: this conclusion depends certainly on the sampling of the considered instruments and should not be stated in general. By the way, are the authors aware of any more or less recent publication where the residual method was applied to the monthly mean values? Isn’t the recommendation not to combine the monthly mean values too obvious for the scientific community for now? Another point to this topic, as shown in Fig. S4 of the paper, there is quite a strong difference between the tropospheric ozone values calculated from daily means and from the collocated data. Thus, the recommendation given by authors to use the daily measurements can be confusing for the readers forcing them to prefer daily means to the fully collocated measurements.
- Page 11, line 271: please provide a reference discussing the OMI row anomaly
- Page 11, lines 274: “region between the two ozone jumps is removed” - from the text above it is unclear which two ozone jumps are meant.
- Page 11, Figure 5: it is not quite clear if the plotted “random uncertainties” are the same as the “uncertainty of the total ozone column” given by Eq. (1)
- Page 12, lines 289 - 293: The logic of these two sentences is not clear. It is unclear how the described procedure “we first create the $1^\circ \times 1^\circ$ gridded and interpolated dataset of ozone profiles, and then we compute stratospheric column via integration of ozone profiles” can mitigate the issue that “the limb instruments have limited accuracy and highly non-uniform coverage in the UTLS”. I can imagine that using multiple instruments might reduce the non-uniform coverage but I doubt it can significantly increase the accuracy. Please comment on that.
- Figure 7: I do not see any stars in the plot.
- Figure S5: It is not quite clear if the differences were interpolated or these are the differences between interpolated MLS and SILAM. Interpolation rule should be reported.
- Figure S6: It is not quite clear why a data assimilation should result in an artificial trend. Could you add the third panel to the figure showing the trends in the assimilated data? Otherwise, the conclusion about a disadvantage of the assimilation looks poorly justified.
- Sect. 4.3.3: The first sentence is misleading as it refers to the region below the tropopause. First, the profile values below the tropopause are not of interest as they are not accounted for when calculating the stratospherical ozone column. Second,

as stated below, the correction is made between two fixed pressure levels having no relation to the real tropopause height.

- Figure 9: 200 hPa and 400 HPa levels should be marked in the plots.
- Figure S12: Sub-optimal color scale. It is almost impossible to estimate the plotted differences. The colors between 10 and 20 DU are almost indistinguishable.
- Page 17, line 394: Please comment on values over Southern America and Africa which seem to be around 10 DU or even larger.
- Page 17, lines 402-403: The correction of 2 DU is quite small and do not significantly change the results, the application of this correction is, however, questionable. This difference can result from an uncertainty in cloud top height definition or from the fact that the clouds are not a purely reflecting layer and radiation penetrates into the cloud to a certain depth. Thus, there might be a physical difference between the integrated limb profiles and total ozone observation in a cloudy atmosphere, which however is not applicable to cloud-free conditions. The correction should be either removed or better justified.

Technical corrections

- Page 1, line 9 (and also Page 2, line 48): “The satellite measurements” → “Satellite measurements”
- Page 1, line 11: “total ozone column” → “total ozone columns”
- Page 1, line 12: “stratospheric ozone column dataset” → “stratospheric ozone column datasets”
- Page 1, lines 14-15: please reword the sentence to avoid a double usage of the word “using”
- Page 8, line 201: extra or missing bracket in “Figure 3, right panels)”
- Page 23, line 477: “However, but the OMI-CCD” → “However, the OMI-CCD”