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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.

**Anonymous Referee #3**

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The manuscript describes a technique to estimate tropospheric ozone columns using limb and nadir measurements from SCIAMACHY and shows comparisons of SCIAMACHY-derived tropospheric ozone columns with those from ozonesondes and other satellite retrievals. The topic is relevant to AMT. The paper could be published in AMT, but only after revision.

Major points:

The estimation of errors is admirable and in some ways goes beyond what has been done in previous studies; however, key assumptions are questionable. Several of the



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error terms given in Eq. 6 will not produce Gaussian errors (for example cross section, aerosol, and tangent height errors). Therefore, using a root-sum-square approach to get an estimate of the total error is not appropriate and will lead to an underestimate of errors. Furthermore, some of the details in the error analysis section need clarification. There is quite a lot of detail given for the calculation of stratospheric errors, while similar detail is not given for the tropospheric column errors and error due to tropopause height specification. Two tables are given for stratospheric error. Instead of providing these errors as a function of height, they should be given for the stratospheric column. A similar table should be provided for tropospheric column errors. More description of the error due to tropopause height should be given. This error appears to be too small as compared with what has been found by others (see Morris et al., http://scholar.valpo.edu/phys_astro_fac_presentations/5/ who found that particular definitions of tropospause heights can produce large differences in tropospheric column amounts found by residual methods).

The latter point raises serious concerns regarding the comparisons between the different methods. A long description is given as to how the tropopause height is computed for the SCIAMACHY retrievals and radiosondes (they are somewhat different in details), but no description is given for the OMI/MLS or TES retrievals. Differences in tropopause definitions can lead to significant differences in computed tropospheric columns. The dependence of tropospheric columns on tropopause height can lead to confusion when comparing different data sets or analyzing the data, particularly in terms of seasonal variability as well as its interpretation (for example, how much of the tropospheric column variation is due to changes in the tropopause height as compared with chemical or transport mechanisms? The latter may be measured with the satellite instruments while the former is not, but is only due to how the specified tropopause changes). To avoid this issue, several studies have instead compared mixing ratios (see for example the Zhang et al., 2010 reference below) or integrated amounts up to a fixed pressure (Schoeberl et al., 2007).



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The comparisons carried out in the paper do not make any attempt to account for differences due to a priori information. This can be accomplished in a straight-forward manner with the averaging kernel approach that is used in TES validation and other papers. The Zhang et al. (2010) reference below shows that when comparing tropospheric ozone data sets with different a priori information and sensitivities, these factors can lead to artificially large differences if not accounted for. The comparisons in the present work leave the reader with questions as to why there are differences and do not provide insight into the differences. Proper account of a priori differences should be done and may decrease actual differences between the data sets, leading to greater confidence in the SCIAMACHY results. As it stands, the comparisons with the other data sets are not particularly useful, particularly within the context of previous results, except to show that basic well-known features are captured by SCIAMACHY (most of the time). However, there are some notable differences between the different data sets as noted in the other reviews, leading to concerns regarding the SCIAMACHY data set.

In figures that focus on seasonal variations, it would be useful to remove biases before comparing the different data sets.

There is no discussion of the treatment of cloudy data in the TES and OMI/MLS data sets. The SCIAMACHY data are filtered to remove cloudy data, so there may also be a clear-sky to all-sky bias. Some discussion on this point must be added.

Please see and reference

Zhang, L., Jacob, D. J., Liu, X., Logan, J. A., Chance, K., Eldering, A., and Bojkov, B. R.: Intercomparison methods for satellite measurements of atmospheric composition: application to tropospheric ozone from TES and OMI, Atmos. Chem. Phys., 10, 4725-4739, doi:10.5194/acp-10-4725-2010, 2010.

The paper contains many grammatical errors and typos; I did not point out all of them (stopped midway through the paper). As this is a major distraction, copy-editing is recommended. Parts of the paper (for example, the introduction) would benefit from



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reorganization to improve the flow. There are also a number of subjective and even incorrect statements. These must be cleaned up before publication.

Technical points:

Sometimes SCIAMACHY is referred to as in the present as in the abstract "which flies as part of. . ." and other times as in the past as in the first sentence of the introduction. As it is unlikely that the satellite will be revived, it would make more sense to consistently refer to it as in the past.

Introduction

1st par.: "Retrieval of...yields important trace constituents" -> "Retrievals from. . . yield important information about trace constituents"

4th par. This paragraph consists of one very long sentence. Suggest breaking it up.

5th par. "Enhanced sensitivity" with respect to what? What does "good global coverage" mean exactly (this is a subjective statement). What is meant exactly by "current generation of sensors"? Does it mean SCIAMACHY and others? If so, say exactly which sensors.

6th par. How do nadir-viewing instruments have a lower probability for cloud interference? Their large fields of view actually have a high probability of cloud interference, but there are established techniques for accounting for the cloud effects. To say these instruments have good horizontal spatial resolution is subjective and some may not agree. Older references (SBUV) exist for this statement and should be added here (they appear later after a discussion of the retrieval of tropospheric ozone by spectral fitting). Again, this paragraph consists of one very long and somewhat unwieldy sentence, so suggest breaking it down.

7th par. Some may not agree that tropospheric ozone has a relatively short lifetime. Please give a range of its lifetime instead. Information about sources and sinks of trace gases is not directly acquired by satellites. Models are needed to help interpret



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the satellite data and derive such information.

8th par. A number of studies have been lumped into a category of “different variants of this method” described in the preceding sentence as “using a combination of two different instruments”. Not all of the methods use two different instruments and not all would qualify as variants. Please provide more distinction.

9th par. 1st sent. Optimal estimation uses everything described in the first part of this sentence and therefore should not be mentioned as a separate technique. Other techniques, such as neural networks, have also been used and should be referenced: see e.g., Sellitto P., B. R. Bojkov, X. Liu, K. Chance and F. Del Frate, Tropospheric ozone column retrieval from the Ozone Monitoring Instrument by means of neural networks algorithms, Atmos. Meas. Tech., 4, 2375-2388, doi:10.5194/amt-4-2375-2011, 2011.

10th par. The first 3 sentences would make more sense if they were placed before the discussion of the spectral fitting. The technique is essentially the same but with a more limited number of wavelengths so that the information content is not as great as that from instruments with more continuous spectral coverage. The 3rd sentence is long and convoluted. The last few sentences belong in a separate paragraph along with the following paragraph. Here the following work should be referenced: Natraj V., X. Liu, S.S. Kulawik, K. Chance, R. Chatfield, D.P. Edwards, A. Eldering, G. Francis, T. Kurosu, K. Pickering, R. Spurr, H. Worden, Multispectral sensitivity studies for the retrieval of tropospheric and lowermost tropospheric ozone from simulated clear sky GEO-CAPE measurements, Atmos. Environ., 45, 7151-7165. doi:10.1016/j.atmosenv.2011.09.014, 2011.

p. 7816, par. 2: I do not believe the first sentence is true, particularly WRT total ozone. Surface reflectivity data bases are typically used for the retrieval of total and tropospheric ozone from UV observations and because they use wavelengths that are not terribly sensitive to the surface. It could be argued that these climatologies do not need to be highly accurate (and here highly accurate needs to be defined). The



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next sentence starts a discussion on thermal infrared techniques and belongs in a new paragraph. Here, references for retrievals from the TES instrument should be included as that instrument was designed specifically to measure tropospheric ozone by resolving absorption lines including pressure broadening effects. The 4th sentence starts a discussion on the use of combined UV/thermal approaches. This could go in a new paragraph. Here, also the above reference to Natraj et al., 2011 should be mentioned as well as Worden, J., X. Liu, K. Bowman, K. Chance, R. Beer, A. Eldering, M. Gunson, H. Worden, Improved tropospheric ozone profile retrievals using OMI and TES radiances, Geophys. Res. Lett., 34(1), L01809, 10.1029/2006GL027806, 2007. In the last sentence, it does not appear that the Bovensmann et al. reference applies to the implementation of the method with GOME-2 and IASI. Here, the following work should also be referenced: Fu, D., J.R. Worden, X. Liu, S. S. Kulawik, K. W. Bowman, and V. Natraj, Characterization of ozone profiles derived from Aura TES and OMI Radiances, Atmos. Chem. Phys., 13, 3445-3462, doi:10.5194/acp-13-3445-2013, 2013.

p. 7816, last par., 2nd sentence: This statement is subjective and it can be argued that it is not true given that many other studies have already characterized stratospheric inhomogeneity and provided global monitoring of tropospheric ozone.

p. 7817, 1st two paragraphs should be joined. 2nd paragraph fragments should be separated by semicolons or periods.

Sect. 2.1 First par. Envisat will not continue to orbit at the stated altitude. Towards the end of the mission, the orbit was already degrading.

p. 7818: Discussion of limb and nadir observation modes would be more clear if contained in separate paragraphs along with another separate paragraph about how they can be collocated. The last sentence of sect. 2.1 belongs at the first part of this section with the discussion of the satellite status.

Sect. 2.2: All instrument abbreviations should be spelled out at their first use. It should be noted that TES discontinued limb mode observations early in the mission.



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p. 7819, bottom: It should be stated that OMI/TOMS is a retrieval of total ozone.

Sect. 2.2: The vertical range and approximate vertical resolution of MLS should be given. A more complete web link to the OMI/MLS tropospheric ozone should be given. From this link, only Tropospheric ozone from the "cloud-slicing" technique is mentioned. If it is OMI/MLS from Ziemke et al. 2006, then the reference needs to be made here.

p. 7820, line 15: carried aloft by

p. 7821, bottom: period is missing for the last sentence.

p. 7823, line 10: The plurality of the subject and verb do not agree and this sentence is awkward.

Sect. 2.3, 1st sent.: The SCIAMACHY LNM retrievals

p. 7824, lines 3-7, this sentence is confusing and is not correct as stated

p. 7826: Discussion of how clouds are handled in the total O3 algorithm is confusing. First, there is a discussion of SACURA, but then it says that clouds are treated as Lambertian without further details. This must be clarified. The discussion on SCODA for the limb mode would more clearly go in a separate paragraph with the 1st sentence of the following paragraph.

p. 7627, lines 3-4: This is not clear. Exactly what kind of sensitivity analysis was conducted and how does the cloud fraction threshold reduce the tropospheric ozone. Is the cloud-shielded ozone not accounted for in the “total” ozone retrieval with the use of a priori information (if so, then it is not really a total ozone retrieval) or is this due to bias in the a priori information?

p. 7827, line 19: impact of albedo errors; line 20: compared with

p. 7828, line 9: This is unclear, is the error in the monthly mean a bias? The paragraph should be broken at line 9 - the start of discussion of errors in the tropospheric column belongs in a separate paragraph.



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p.7829, line 13 and 7830 lines 2 and 23 and 26: plurality of subject and verb do not agree

p. 7830, line 23: typo

p. 7832, Java, not JAVA, line 19: This is not a sentence.

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