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Atmospheric Measurement Techniques Discussions

Interactive comment on "Global validation of improved SCIAMACHY scientific ozone limb data using ozonesonde measurements" *by* J. Jia et al.

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The authors would like to thank Referee #1 for reviewing the manuscript.

Comments: This paper presents a review of the algorithm developments for version 3.0 of the Bremen SCIAMACHY limb ozone retrievals as well as an intercomparison of versions 2.9 and 3.0 with the WOUDC ozone sonde database. Some details in the study are lacking and several comments below need to be addressed. Overall the language of the manuscript needs to be improved. Specific comments: Title: why use the descriptor "scientific"? Also, it might be helpful to mention the version(s) in the title.

Response: The authors agree that the word 'scientific' is not proper. We want to thank the reviewer for correcting. This word was chosen to distinguish our data from the

ESA Off-Line operational data (OL). The title has been adjusted to 'Global validation of SCIAMACHY limb ozone data (Versions 2.9 and 3.0 IUP Bremen) using ozonesonde measurements'

4818-19: At least provide references for the list of satellite instruments, and consider ex-panding the acronym to the full instrument name

Response: The references are added. The sentence has been changed into 'For satellite in-struments, different observation techniques including solar/stellar occultation measurements, e.g. SAGE (McCormick et al., 1989), HALOE (Russell et al., 1994), ACE (McElroy et al., 2007), GOMOS (Bertaux et al., 2010), limb scatter/emission measurements, e.g. MLS (Waters et al., 2006), MIPAS (Fischer et al., 2008), OSIRIS (Llewellyn et al, 2004) and nadir measurements, e.g. GOME/GOME2 (Burrows et al., 1999; Callies et al., 2000), OMI (Levelt et al., 2006), IASI (Clerbaux et al., 2009), are used (see e.g. Sofieva et al., 2013; Hassler et al., 2014 and references therein).'

4819: Include the specific version for which Raphoe et al. provided the error budget. Is there any change in the error budget due to the changes in the algorithm for Versions 2.9 and 3.0?

Response: The specific version 2.5 has been added. The text has been changed to ' The error budget typical for limb ozone V2.5 data set was reported by Rahpoe et al. (2013).' V2.9 is a reprocessing of V2.5, the error budget is not expected to change in V2.9. So far we haven't calculate the error budget for the new version V3.0 as the retrieval algorithm need to be optimized as mentioned in the conclusion.

4819: Are there any differences in forward model parameter assumptions between the two versions, i.e. surface albedo, aerosol, etc.?

Response: Yes aerosol and surface albedo have been changed. In V2.9 the ECSTRA (Fussen et al., 1999) aerosol data base was used; the surface albedo was from Matthews (1983). In V3.0 the aerosol was retrieved from SCIAMACHY (Ernst et al.,

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2012); the weighting function of the surface albedo was included in the fit (See P.4823 L.23). To include these information into the manuscript, the authors have added '..with the radiative transfer model SCIATRAN (Rozanov et al., 2014). In V2.9 retrieval, the ECSTRA (Fussen et al., 1999) aerosol data base was used in the model; the surface albedo was from Matthews (1983). The lowest and highest ...' in P.4822 L.15; and '...is also included in the fit. The aerosol extinction coefficients retrieved from SCIAMACHY limb measurements (Ernst et al., 2012) are used in the forward model. The mathematical inversion then processds ...' in P.4823 L.24.

4819: What does "+5%" mean?

Response: The +5% means positive bias of 5% which is used in the cited paper. We changed it into '5%' in the new manuscript to avoid confusing.

4819: Previous comparisons to ozone sondes are mentioned – what is the reference to this work? It should be referenced in detail here.

Response: The previous comparisons are done by the author as a preliminary result of this work. The sentence has been changed into 'Our comparisons of this data set (unpublished) to ozone sondes ...'

4819: The effect of the stray light is speculative, which is interesting, but please provide some rationale as to how this is actually affecting the retrieval. It is not clear why the DOAS approach would necessarily be better in this scenario. Did the authors test the effect of the assumed aerosol load? This would also be sensitive to changing solar azimuth angles and have the strongest effect for small scattering angles. This needs to be addressed, and a systematic study on this is likely warranted.

Response: Please keep in mind that the authors are not sure whether the stray light is the 'real' reason but a 'possible' reason. The stray light effect is not fully understood so far. One explanation why the DOAS approach would be better is that: the stray light follows the shape of the spectrum, when applying the DOAS approach the effect

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is substantially alleviated by a closure polynomial. This information has been generally discussed in the second paragraph of the section 4.3. We did test the aerosol influence in the northern high latitude. It has indeed stronger effect for small scattering angles, however the overall influence is not high. The results show increasing ozone number density with higher aerosol load and this can't explain the sever underestimation.

4821: An additional figure showing the relative azimuth angle as a function of angle along the orbit track would be helpful (in the text the angles are only referred to "small" and "large"), as this is an important aspect of understanding the biases in the retrieval. Also, "solar light" is not a typically used phrase.

Response: We have added a reference for the figure required. The manuscript has been changed into '... from behind the instrument field of view and the relative azimuth angle is large (See Fig. 4.3 and Fig. 4.4 in Ernst, 2013).' The word 'solar light' has been changed to 'sun light'.

4822: What are the "issues" near the boundary of channel 3? Specific calibration problems?

Response: The 'issues' for each boundary of the channels are the same: the sensitivity of the dichroic mirror. The dichroic mirror is used to filter the corresponding wavelength at each channel. At the boundary of the channels, the mirror will accept part (sensitive to temperature gradient etc) of the out-range wavelength light and reflect part of the in-range wavelength light therefore cause calibration uncertainties. The calibration problem is now specified as '...because of large calibration uncertainties near the boundary of SCIAMACHY channel 3 related to the dichroic mirror.'

4822: How is the extra-terrestrial solar spectrum obtained? Is it measured, or from a stan-dard database?

Response: It is measured. To specify that the text has been changed to '..., the extra-terrestrial solar spectrum measured once per orbit by the SCIAMACHY instrument is

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used ...'

4823: How is the wavelength dependence of the surface albedo known?

Response: Thank you for the correction. There is no wavelength dependence. This sentence should clarify the difference of the surface albedo in forward model. The proper description is 'The weighting function of the surface albedo is also included in the fit.'

4824: At what altitude does the UV information begin to have a significant effect in the version 3.0 retrieval?

Response: The UV information only contribute from ∼35 km upper for ozone retrieval. This work focus on comparisons with ozone sondes which only reach ∼30 km. Hence the UV retrieval influence is not discussed here. In the manuscript the related sentence has been changed into '... from the UV range does not have any significant influence on the retrieved ozone values below 30 km (influence above ∼35 km)' .

4824: What is a "ground pixel"? This is confusing.

Response: The word 'ground pixel' has been replaced by 'measurement'.

4825: How sensitive are the results of this study to the choice of coincidence criteria?

Response: We have chosen this coincidence criteria to maximize the data amount. According to the experience the authors gain from other satellite data, by changing temporal and spatial criteria within our range the changes of the results would be around 2 % in the lower stratosphere. See Studer et al., 2013 (doi:10.5194/amtd-6-6097-2013). The shape of the averaged profiles would be similar but a bit rougher. However, we didn't test the sensitivity caused from the coincidence criteria by ourselves since our loose criteria worked quite well already.

4826: Section 3.1.1 is not rigorously explained and the wording is confusing. Please revise. Particularly, the sentence beginning "This represents the profiles ... " is unclear

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and confusing.

Response: This part is revised to ' To make a quantitative comparison the ozone sonde data are degraded to the vertical resolution of the satellite data. To this end the a priori profiles and the rows of the averaging kernels from the SCIAMACHY retrieval are resampled to the vertical grid of the sonde data. The elements of the resampled a priori profiles and averaging kernels are denoted as Aij and Xaj respectively. The low resolution ozone sonde profiles are obtained then as follows: Equation (4) where i represents the satellite coarse grid, and j the fine grid of the ozone sonde; delta Zj is the altitude interval, i.e. a half distance between the layers above and below j.'

4827: What is an "averaged altitude between January 2003 and December 2011"? Also very confusing.

Response: To clarify it, the second paragraph of P.4827 is revised to ' V3.0 profiles are retrieved at the measurement grid with altitude levels varying depending on the location and time. To obtain a common altitude grid for the mean profile, all altitude levels which belong to a certain elevation step are averaged over the whole measurement time (Jan 2003 to Dec 2011). Then each single profile is ... '

4841: Why is the altitude range so different between versions 2.9 and 3.0?

Response: The comparison results are showed after screening according to Sect. 3.1.2. These two criteria are responsible for the different altitude range at upper and lower parts, respectively. Explanation for the first criterion: The ozone sonde data has to be degraded in the vertical resolution and resampled to coarser vertical grids of V2.9 (1km) and V3.0 ($\sim$3.3km). This work is explained in P. 4826. Please notice that the average kernels are used in the process. One criteria for degrading and resampling of the ozone data at each particular altitude layer is that: the AK curve must be within sonde altitude range, in other word, 'must not have non-zero elements above the maximum height of the corresponding ozone sonde measurement', otherwise it will require ozone sonde information beyond its maximum height, which obviously we don't have.

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The consequence will be, for instance, if one ozone sonde profile originally ends at an altitude of 34 km, while V3.0 has tangent heights of ... 29 km, 32 km, 35 km ..., the last sonde layer that is calculated would be at 29 km instead of 32 km, because at 32 km the calculating will still need ozone sonde measurement until 35 km to convolve with the corresponding AK curve (see Fig. 1) to guarantee a correct result. However, for V2.9 the last layer of the corresponding sonde would be higher resulting from a finer vertical step. So it is very common to see the upper altitude of V3.0 lower than in V2.9 with about 3 km differences. The lower part altitude difference, however, is caused by the vertical resolution criterion. The latter is determined by the retrieval sensitivity and is different for V2.9 and 3.0. For instance, at station Nairobi, the V2.9 vertical resolutions are higher than 6 for the latitude range 10-18 km. To offer more details, the author has changed the text at P. 4827, L. 3-8 into 'Firstly, ... of the corresponding ozone sonde measurement. Due to a coarser altitude grid in V3.0, a wider vertical range is rejected when excluding one altitude level, which causes different altitude range at the upper altitude in Figs. 2-7. Secondly, ..., are also not considered. Since the vertical resolution is different between the two versions, some differences in altitude range are expected.'

4828: It is unclear if the statistical comparisons (Figs 2-8) are done on the average profile or on the set of coincident profiles. Please clarify.

Response: Sentence at P. 4828 L. 13-14 has been changed into 'Average vertical profiles from the coincident SCIAMACHY and ozone sonde measurements at the six selected stations ...'

4829: Why is the overestimation at the ozone peak in the tropics not shown?

Response: The work involved more than 60 stations as mentioned. To fit the main aim of the manuscript - show the general result of the validation, six typical stations are chosen to represent different latitude bands as shown in Figs. 2-7. In the tropics, Nairobi represents the common situation. Although the overestimation we mentioned is not showed in the Figs. 2-7, it is demonstrated in Fig. 8 upper panel. The author has

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changed the wording 'not shown in this paper' into 'See upper panel of Fig. 8'.

4830: What is the main reason for doing the partial column comparison in addition to the vertical profile study? Is there really any information to be gained? Please specify.

Response: The partial column comparison is done in the need of the tropospheric ozone retrieval by using Limb Nadir Matching method. The limb data column is an important input for the tropospheric ozone data retrieval. It is one of the main factors determining the tropospheric ozone data quality. The manuscript has been edited to specify this reason in the beginning of section 3.2 in P.4828 as 'Apart from vertical information, the limb partial columns are used for tropospheric ozone retrieval using residual method (Ebojie et al., 2015). Therefore, in addition to the vertical profile comparison, the results of partial column comparisons ... '

4832: Is it possible to show a plot of the radiances to identify the stray light contamination? Again, could the authors explain how the stray light could impact the peak value (only?) of the retrieved profile? Did the testing with synthetic retrievals including a full range of for-ward model parameters, including a range of aerosol, surface reflectance, etc.?

Response: It is possible to show a plot, however, the authors think this plot will not fit the main topic of this manuscript, which is validation. The stray light didn't only impact the peak value. Please note that Fig. 19 shows the differences over a 10 km range (12 km - 22 km). It is only more obvious near the altitude of the peak value, which is taken as an example in the discussion. Also, please keep in mind that the authors are not sure the stray light is the 'real' reason but a 'possible' reason. The stray light effect is not fully understood so far. The synthetic retrievals were done using a full range of forward model parameters. The re-lated sentence in the manuscript (P. 4832 L.10) has been modified into 'As this artifact could not be reproduced in the synthetic retrievals, which include a full range of forward model parameters, it is most probably caused by instrumental issues.'

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