

Interactive comment on “Comparison of OMI ozone and UV irradiance data with ground-based measurements at two French sites” by V. Buchard et al.

V. Buchard et al.

Received and published: 29 May 2008

Answers to referee #1 comments, received and published on 17 April 2008, on the manuscript :

"Comparison of OMI ozone and UV irradiance data with ground-based measurements at two French sites."

General comments:

The manuscript presents a comparison of ground-based measurements of ozone and UV irradiance at two French sites with OMI satellite estimates. This kind of comparison is a complicated but useful task, since the spatial distribution of solar UV irradiance

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received at the ground is mainly controlled by the variability of total ozone and clouds. The manuscript addresses relevant scientific questions within the scope of ACP, but major revisions are needed before final publication.

Specific Comments:

1. Numbers, describing more explicitly the differences between the ground-based measurements and the satellite estimates, should be mentioned in the abstract and conclusions paragraphs, derived (at least for ozone) from Tables 1 and 2. A significant lack of quantitative results is observed also in the results paragraph.

Reply: According to the reviewer suggestion, we have improved the abstract, the paragraph 3 "Results" and the conclusion with more numbers for the results.

2. The comparison of ground-based and satellite ozone and UV data has been extensively studied during the last 15 years. In contrast, only two references (corresponding to UV effects and instrument uncertainties) are reported in the introduction paragraph. This paragraph should be extended, in order to give proper credit to related studies and present some of their main findings. The reviewer could propose the recent studies of Balis et al., (2007), Kazantzidis et al.,(2006) and all references therein.

Reply: Following the reviewer comment, the references of Kalliskota et al, 2000; McKenzie et al, 2001; Cede et al, 2004; Arola et al, 2005; Kazantzidis et al, 2006 have been added in the introduction paragraph in the new manuscript. Their results are commented in paragraph 3.2 "UV comparisons". The reference Balis et al, 2007 has been added and commented in the paragraph 3.1 "Ozone comparisons",page 4316.

3. Page 4312, lines 22-24: the authors compare momentary ozone retrievals from OMI with daily averages of ground-based data. In case that it is possible, average values of the total ozone column, close to the satellite over pass time, should be used for this study.

Reply: We have made the two comparisons: momentary TOC from OMI with daily

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averages of the TOC from ground based measurements and also with average values close to the satellite overpass time. The results were similar for the two comparisons. We choose to show the comparison of daily averages of the TOC for ground-based. We have explained that in the new manuscript.

4. There are too many figures and they do not discussed extensively. Figures 1a, 2a, 2d, 3a, 3b, 4a, 4b, 4d, 5a and 5c could be omitted. The results of those figures could be easily described in the text and presented in a table. In addition, the percentage differences between the ground-based and the satellite data could be still observed and examined as a function of quantities like SZA, Julian Day etc., using the rest of the figures.

Reply: We choose to keep the figures. In other papers such as Arola et al., 2005; Cheymol et al., 2006; Tanskanen et al., 2007, regression lines are showed. We think it is much clearer.

5. Page 4316, lines 6-14: The authors report that the percentage difference between total ozone values from the spectroradiometer and OMI has not any seasonal dependence for clear sky data. In addition, they report that the dependence with solar zenith angle is small. According to the opinion of the reviewer, there is a seasonal dependence, although it is not as obvious as at VdA (figure 2b). And, is there any explanation for this result? The satellite estimation of total ozone derived from the same algorithm that was used also for TOMS. In this case, a comparison of results from previous studies at other sites (or even at this site) could be further discussed.

Reply: We agree that there is a small seasonal effect for clear sky data in Briançon, smaller than for VdA. We have searched for an explanation but we have found none.

A comparison with TOMS TOC at Villeneuve d'Ascq by clear skies has been made in Houët and Brogniez (2004), there was a small bias ($VdA > TOMS$) but no seasonal effect was observed.

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6. The previous comment responds also to all results derived from the validation of all UV satellite products; further and in depth discussion of the outcomes of this manuscript should be added.

Reply: We agree, see reply to comment 2.

7. Section 3.2: At least for the UV quantities, the percentage difference between the satellite and the ground-based measurements should be normalized with the satellite estimates and not with the ground based measurements. The reason for this was extensively discussed in Fioletov et al., (Optical Engineering, 2002, pp. 3057-3058). The authors are encouraged to adapt that aspect of validating UV satellite data and perform new calculations.

Reply: We choose to normalize the percentage difference between the satellite and the ground-based measurements by the ground-based to be consistent with other works (Arola et al, 2005; Cede et al, 2004). We prefer to keep the ground-based reference for all comparisons, including TOC.

8. Page 4318, lines 17-19: it is reported that there is no correlation between the distance of OMI pixel and the VdA site. At some sites there is a correlation with the geographical position of the OMI pixel relative to the site (if it is at the North, South, East or West). A possible correlation (derived as an impact of topography) could be examined.

Reply: In Villeneuve d'Ascq the ground surface is flat, and we have found no dependence with the position of the OMI pixel.

9. Page 4319, lines 8-12: the problem of the TOMS (and OMI) algorithm in distinguishing between snow cover and clouds has been discussed also in other studies (e.g. Fioletov et al., 2004, Kalliskota et al., 2000). The authors could compare their findings and extend the discussion also on this topic, using the outcomes of the previous studies.

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Reply: The references of Kalliskota et al., 2000 and Krotkov et al., 2001, 2002 have been added in the paragraph 3.2.3 "Erythema daily dose" with the comment "Past studies have also revealed this underestimation of the surface UV provided by TOMS data."

10. Page 4320, lines 14-16: the reduced effect of the SZA effect in the new version of data has not been discussed in the previous sections. A proper documentation or reference could be added or the sentence could be omitted.

Reply: Pepijn Veeffkind has informed us of the improvement in the new version. This problem is discussed in a paper to be published soon (Kroon et al, 2008 JGR in press). We think it must be mentioned in our manuscript.

Technical corrections:

- Page 4310, line 3: replace "things" with "atmospheric and radiometric quantities".

It has been done.

- Give full names for the acronyms NPL, NIST, QASUME, FWHM.

The full names for the acronyms have been added.

Interactive Comment :

- Page 4312, line 2: please provide some references about the QASUME project results

It has been done.

- Page 4312, line 9: please specify the COST number (726?) and provide some references about this action and the August 2006 campaign.

Following the reviewer#2 comment, we have removed the comparison with the radiometer, so information on COST are not necessary.

- Page 4310, line 11: the institute where the instrument belongs is not necessary to be included in the abstract, so the sentence could be omitted.

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We agree, this sentence has been moved in the acknowledgements section.

- Page 4313, line 2: replace 'rate' with 'rates'.

The correction has been made

- Page 4313, line 3: the reference of Diffey and McKinlay is missing.

The reference has been added

- Page 4317, line 27: the reference of Brogniez et al., 2008 is missing.

We don't understand, the reference is already in the list.

- Page 4319, line 19: Table 2 is referenced here, but the statistical quantities are explained in the summary section. The text of the manuscript or the Table captions should be modified accordingly.

We agree, for clarity we have removed the references to Table 1 and 2 in all sections except the summary section and we have defined the statistical quantities in Table 1 caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4309, 2008.

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