

Interactive comment on “Measurements of UV irradiance within the area of one satellite pixel” by P. Weihs et al.

Anonymous Referee #2

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Review of "Measurements of UV irradiance within the area of one satellite pixel" by P. Weihs et al..

General comment:

This paper concerns the analysis of erythemal UV measurements obtained in several sites during a campaign conducted to estimate the homogeneity of the UV field within the area of a satellite pixel. A comparison of erythemal UV derived from OMI with the previous ground-based measurements is carried out. The aim of this work is to assess how the satellite data are representative of the UV at the surface. This kind of study is essential to enable the end-user of satellite data to know if he/she can have confidence in these data. The results and the conclusions of the analyses are very interesting but

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a study extended to the winter season would have been useful to estimate also the effect of the surface albedo.

Summary:

- 1) Does the paper address relevant scientific questions within the scope of ACP? Yes
- 2) Does the paper present novel concepts, ideas, tools, or data? Yes
- 3) Are substantial conclusions reached? Yes
- 4) Are the scientific methods and assumptions valid and clearly outlined? Yes
- 5) Are the results sufficient to support the interpretations and conclusions? Yes
- 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No
- 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes
- 8) Does the title clearly reflect the contents of the paper? Yes
- 9) Does the abstract provide a concise and complete summary? Yes
- 10) Is the overall presentation well structured and clear? Yes
- 11) Is the language fluent and precise?
- 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes
- 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes
- 14) Are the number and quality of references appropriate? Yes
- 15) Is the amount and quality of supplementary material appropriate?

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Specific comments:

- p. 3695, line 16: the paper Tanskanen et al, 2008, does not concern TOMS.
 - p. 3697, line 6: 'Station Bad Voeslau is already ...', what does 'already' mean in this sentence?
 - p. 3697, lines 7&8: 'It could be used ... in Vienna'. What is the aim of such a sentence since finally, as I understand BOKU is chosen as the reference?
 - p. 3697, line 15: it would be interesting to compare these cloudiness conditions to the cloudiness flags from OMI to obtain info on cloudiness over the other sites.
 - p. 3698, lines 7&8: Fig 3 compares AOD at BOKU and TGM. In 2.1 it was not stated that TGM was equipped with a sunphotometer. The authors say that the figure shows no 'distinctive difference between...'. It is optimistic. Without giving the uncertainties on AOD it is impossible to make such a conclusion. The authors must give the AOD uncertainties.
- Why the authors do not compare the AOT at 368nm that is an UV wavelength, rather than at 500nm? (see also below).
- p. 3698, line 20: the ozone data are taken from OMI, that means they correspond to average values over the pixel, not to the true values over each site. It would be interesting to estimate the impact of this approximation on the homogenization performed.
 - p. 3698, line 24: here the wavelength used is 368nm. See the remark above.
 - p. 3699, line 7: which data are comparable to the clear-sky data ? To support the discussion that follows it would be better to show a figure.
 - p. 3700, line 1: 'around a mean systematic bias', Rmean is rather only 'a mean value', and doesn't show always a bias. Moreover 'systematic bias' looks like a pleonasm.
 - p. 3700, line 8: it is not clear for me if Fig 5 shows the variability for clear-sky, partly

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cloudy and overcast because there are only 2 plots without a label and there is no explanation in the caption.

- p. 3701, the numbers of events given in lines 7, 8 are repeated in line 11 and also p. 3702 in lines 6 and 18. Please avoid such repetitions.

- p. 3702, lines 1-2: I am not convinced that the discrepancy between OMI UV and ground-based measurements seems larger for $SZA > 30^\circ$. Again uncertainty estimation on the ratios is needed. The AOD is at 368nm, that confirms that Fig 3 would show that AOD rather than AOD at 500nm.

- p. 3704, lines 1-3: under partly cloudy conditions more data on COD are not sufficient to test the OMI algorithm, it is necessary to know if the sun is hidden or not.

Technical corrections:

- The affiliation of A. Tanskanen seems strange.

- p. 3695, line 29: 'No studies have' -> 'No study has'.

- p. 3700, line 22: 'quiet heterogeneous' -> 'quite heterogeneous'.

- p. 3702, lines 9: 'are one average' -> 'are on average'.

- p. 3704, lines 8: '5 UV index und overcast' -> '5 UV index under overcast'.

Table 1 and Fig 1: the latitude given in the table for Bad Voeslau seems to be too high, it does not fit the position of site 5 in Fig 1 (the green dot 5 is below the rectangle on my computer).

Table 2: the same info is given on the two sides apart from the first diagonals (except the sign of course). This must be avoided.

Fig 4: in the caption '367nm' -> '368nm'.

Fig 5: see comment above concerning p. 3700 line 7, in the caption there is no explanation about (a) and (b), and there is no (c). 3rd line of the caption: '1130' -> '11:30'.

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Fig 6: the caption states that it is the ratios of ground measurements to OMI, while in the y-axes it is written OMI to ground station.

I am surprised with the words 'momentary' or 'momently' used throughout the text. Could you check their usage?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3693, 2008.

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