

## ***Interactive comment on “Validity of satellite measurements used for the monitoring of UV radiation risk on health” by F. Jégou et al.***

**F. Jégou et al.**

fabrice.jegou@cnrs-orleans.fr

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Answer to the anonymous referee #1

We thank anonymous referee #1 for his/her helpful comments. Please find below our answers. All specific comments (in italic) have been taken into account. Subsequent modifications in the text are highlighted in bold.

Specifics comments

Referee #1: p. 17378, line 22: I suggest to clarify that UVI and erythemal dose rate are basically the same quantity: “one unit equals to erythemal dose rate of 25 mW m<sup>-2</sup>”

Following the referee suggestion the text has been modified: **“The UVI is a di-**

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dimensionless quantity, where one unit is equal to an erythemal dose rate of 25 mW.m<sup>-2</sup>Åž.

Referee #1: p. 17380, line 20: Please add the version number of the OMI UV product: You seem to be using the current version, which is version 3 (also known as collection 3) data. Version numbers are important for future references. I appreciate that you have added the version numbers for SCIAMACHY and GOME-2 products.

Following the referee suggestion the version of OMI products has been included: ÅžThe OMUVBd (version 3) data product selected are TOMS-like daily L3 gridded (lat-lon 1°x1°) data product.Åž

Referee #1: p. 17385, lines 24-25: You state that the monthly climatological aerosol correction of the MOCAGE model is consistent with the approach in the OMI products. But in line 14 of page 17380 you mention that the current OMI algorithm does not account for aerosols. How can this be consistent?

Thanks to the referee for detecting this inconsistency. Indeed, the two approaches are really similar in not taking into account the absorbing aerosols that leads to an overestimation in the urban regions, therefore the two sections have been modified..

The line 14 p. 17380 has been rewritten: “The current algorithm (Krotkov et al., 1998, 2001, 2002) does not account for absorbing aerosols (e.g. organic carbon, smoke, and dust) or trace gases (e.g., NO<sub>2</sub>, SO<sub>2</sub>), which are known to lead to systematic overestimation of the surface UV irradiance (Chubarova, 2004; Arola et al., 2009) and neglects the cirrus effect on UV radiation. The OMI-derived surface UV irradiances are expected to show overestimation for regions that are affected by absorbing aerosols. Greatest overestimations are anticipated for regions affected by urban pollution and for major natural aerosol episodes.”

The lines 24-25 p. 17385 have been rewritten: “This parametrisation is consistent with the approach for the OMI UVI product for instance. The current OMI surface

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UV algorithm applies no absorbing aerosol correction that overestimates the UVI in polluted region.”

Referee #1: 17388, line 15: You state that there is a systematic high bias of ca. 1 UVI in OMI products compared to SCIA and GOME-2 products. This might be true but the evidence given in the previous paragraph is based only on the difference in annual maximum values. This is surely not enough to detect any systematic bias in the product. For the RISC-UV data (p. 17392, line 26) you obtain a similar bias of 0.2 UVI for both OMI and GOME-2 products compared to the spectrometer, indicating that there is no significant bias between OMI and GOME-2 in this period. To provide convincing evidence for the systematic bias based on the annual data, provide difference plots of the data in fig.2 for OMI-CS – SCIA and in fig 3. for OMI – GOME-2, together with the values of the absolute mean differences for the two cases. Alternatively, I suggest to remove the last paragraph of section 5.1 (lines 15 -18 in page 17388) completely. The second sentence of this paragraph (lines 17-18) is irrelevant because the differences between the satellite products can be studied without the ground-based data.

We agree with the referee and the differences (OMI-CS – SCIA and OMI – GOME-2) have been added to the figure 2 and 3.

The paragraph has been rewritten: This section is focused on the annual UVI cycle observed over the SIRTAs observatory by the three satellite-borne instruments. Figure2 shows the clear-sky products OMI-CS and SCIAMACHY, and Fig.3 the all-sky products OMI and GOME-2 during 2008 and 2009. The absolute mean difference of the satellite products is also shown in Fig.2 (bottom panel) and Fig.3 only for 2009 year (bottom panel).

3 new figures have been added: - figure 2 e,f: annual absolute difference between OMI-CS and SCIAMACHY (2008, 2009) These two figures are added below the figures 2a, b, c, d with 2008 on the left and 2009 on the right. - figure 3 d: annual absolute difference between OMI and GOME2 (2009). The figure 3 c (figure with “NO DATA”) is

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removed. The figure 3 a, b, c and d are now placed in the same column.

figure 2 old organization: a b figure 2 new organization: a b c d c d e f figure 3 old organization: a b figure 3 new organization: a c d b c d

New caption of the figure 2: SCIAMACHY and OMI (clear-sky data) satellite 2008 (left) and 2009 (right) UVI products. Bottom figures: absolute mean difference between OMI clear sky and SCIAMACHY products for 2008 (left) and 2009 (right). Red squares: RISC UV campaign periods.

New caption of the figure 3: OMI and GOME2 (all-sky data) satellite 2008 (left) and 2009 (right) UVI products. Bottom left figure: absolute mean difference between OMI and GOME2 products for 2009. Red squares: RISC UV campaign periods.

In agreement with the referee comment, the following paragraph has been removed (lines 15-18 in page 17385): “This comparison of the annual UVI cycle highlights a systematic high bias of 1 UVI and  $<1$  UVI, in the OMI products compared to the SCIAMACHY and GOME-2 products, respectively. The comparisons of the satellites and RISC-UV ground-based products are a good opportunity to evaluate the reality of such differences.”

and replaced by this paragraph:

“This comparison of the annual UVI cycle highlights a systematic annual bias of  $\sim 0.6$  UVI in the OMI-CS products compared to the SCIAMACHY products. This bias is dependent of the seasonal cycle with value of  $\sim 0.52$  UVI in the September–October period and  $\sim 1.1$  UVI in the May–June period. The absolute mean differences between OMI and GOME 2 from June to December 2009 (bottom panel of Fig. 3) highlight a good agreement between their products. No annual mean difference have been found between these two products. The mean difference in June, 2009 is only 0.2 UVI.”

Referee #1: “p. 17388, line 27, and fig. 4: You mention, both in the main text and the figure label, that in fig. 4 the black symbols represent the UVI products, including

GOME-2. I cannot see a symbol for GOME-2 in fig.4. Please add it.

Following the referee suggestion the GOME2 reference has been removed p 17388, line 27 and in figure 4, because no GOME2 data is available for May 22 and 27, 2009.

Referee #1: Fig. 10, label: Please do not mention SCIAMACHY in the label as it is not shown in the figure. Please add an explanation on what the error or variability bars represent in the middle and right panels (is it one standard deviation of the binned data?).

Following the referee suggestion the reference to SCIAMACHY has been removed in Figure 10 and we have completed the caption: "Evolution of the ratio of the UVI observed and modelled to the UVI modelled on clear-sky conditions with the TUV model, as a function of the cloud coverage. Comparisons between satellite UVI products (OMI, GOME2), MOCAGE model, spectrometer and the average of SIRTA instruments (pyranometer UVAE, pyranometer UVB, biometer) during RISC-UV campaigns for individual data (left) and data binned in octal intervals (middle and right). The error bars shown in the middle and right panels represent  $\pm$ one standard deviation of the binned data."

Referee #1: p. 17395, lines 17 - 29: For me, fig. 10 is the most interesting plot because the cloud effect is the key issue in this paper. Therefore, the paper would be stronger if quantitative evidence based on 3D radiative transfer modelling could be shown for this qualitative explanation here. However, radiative transfer simulation of broken cloud fields is such a difficult task that I do not request for it unless you have such a capability readily available.

Author: "We agree that cloud effect on measured UV radiation is an important issue in our paper. However, there was no modelling capability for 3D radiative transfer simulation of broken cloud fields in the RISC-UV project. Such simulations could be made in future projects."

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Referee #1: 17397, line 18: How do you know that all these new climatologies will be reliable? Have they been thoroughly validated? Please provide some evidence or remove the word “reliable”.

Author: We agree that these climatologies have not been fully validated. The text was modified as follows: “All these climatologies will be a new source of information for epidemiologists to evaluate the UV-risk on health.”

Technical comments

They have all been taken into account.

Answer to the anonymous referee #2

We thank anonymous referee #2 for his/her helpful comments. Please find below our answers.

Specific comments

Chapter 1 'Introduction' Referee #2: “The Introduction contains many well known (and true) facts and general statements but mainly without a cognizable link to what has been done -namely the evaluation of the consistency between UV modeling, UV measurements from satellites and ground instruments- and why this has been done. Half of the introduction addresses definitions of UV-A, UV-B, and UV-C, the description and spectral effects of an action spectrum, or gives more qualitative statements like 'exposure to UV can be deleterious or beneficial'. The introduction would be more path-breaking if already here the authors could describe more concretely and comprehensibly why an evaluation of the consistency between UV modeling, UV measurements from satellites and ground instruments contributes to a project that aims to study the link between increases of cutaneous cancer, atmospheric effects of UV-radiation, and human behavioral factors. For example, formulating one or more concrete open question(s) could be an 'appetiser'.”

We agree with referee 2, some information contained in the introduction do not con-

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cern directly the article. But this information deals with the framework of the RISC-UV project: the UV risk on health. This project leans on the scientific collaboration between a medical community and atmospheric physicists. We expect that physicist readers will be interesting by the health side of our project and will estimate this additional medical information relevant. No medical information is included in the core of the article that is the reason why we keep this information in our introduction.

The first paragraph of the introduction has been reduced to avoid repetition. Following the referee suggestion, the objective of the article has been clarified with a concrete question in the fourth paragraph of the introduction: “Only satellite observations can provide a sufficient spatial and temporal coverage of the global population. Nevertheless, are satellite data accurate enough to help the medical community make public recommendation about UV radiation? The objective of this study is to answer this question.”

Chapter 8 'Discussion' Referee #2: “However, the paper contains several redundant text elements and paragraphs. It would benefit from being written more concisely in the sense that the special findings of this study could be brought more into the focus. This means to become more consistent with the expressed tasks of presenting and evaluating the consistency between model results, satellite and ground based measurements. Especially, this concerns the chapters Introduction, Discussion and Conclusions.Â&Â&Z

We agree with referee 2 comment, some redundant information is present in the discussion/conclusion. The third and fourth paragraphs of the discussion section have been deleted.

Chapter 9 'Conclusions' Referee #2 : “Analysing the intercomparison results the authors came to the conclusion that is is difficult to account for the great temporal and spatial variability of cloud cover into retrieval algorithms or models. Certainly comprehensible but, when regarding all the papers related to UV within the last ten years, not a very new finding. This could be relativised.”

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We agree with referee 2, the difficulty to account for the great spatial and temporal variability of clouds is not a new scientific result. We have relativized this conclusion in changing the following sentence in the second paragraph: “This intercomparison has pointed out the difficulty to take into account the great...” by the sentence : “This intercomparison has confirmed the well-known difficulty to take into account the great...”

Referee #2 : “Furthermore, the authors state that the quality of cloudiness forecast by the numerical MOCAGE model is insufficient and that MOCAGE and OMI products do not come to the same conclusions in terms of cloud cover. The authors mention that an investigation of this discrepancy and its correction will improve forecast models. This is certainly true, but an outlook to the future and a rather general statement. What would be an adequate and target-oriented approach here? On the other hand the authors state in the next paragraph: ‘From now on, UV monitoring can be done by using satellite products (OMI, GOME2) and the UV forecasts by using modeling’. Initially this phrase was somehow confusing to me.”

The strategy to improve the MOCAGE UV forecasts is not yet clearly defined. The next improvement will consist in taking into account the aerosols distribution also computed in MOCAGE in the UVI calculations. Following the referee suggestion, we have been added in the third paragraph this future development: “A first improvement will consist in taking into account the aerosol distribution also computed in MOCAGE in the UVI calculations.”

The fifth paragraph has been deleted to avoid repetition.

The sixth paragraph has been moved to the end of the third paragraph to gather information about the MOCAGE model.

The last paragraph has been rewritten and divided in two parts, paragraph 5 and paragraph 6. The paragraph 5 is a rewriting of the previous last paragraph:

Paragraph 5:

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In order to make sensible choices on sun exposure, the public needs more understandable information. The public needs knowledge of the UV environment as it relates to sunburn and to vitamin D production. Currently, the necessary information is not generally available to the public. In the case when UV information is available (e.g. in France by Météo-France), new agencies and newspapers broadcast this information only during the summer period. Further, often only the peak daily value is provided. This is because the UVI was originally designed only to give the risk of skin damage. When the UVI is 3, skin damage occurs after approximately 1 h, but sufficient vitamin D can still be produced in a few minutes provided face, arms and legs are exposed (McKenzie et al., 2009). For the public to be able to make informed decisions about appropriate solar behaviour to avoid skin damage in summer and vitamin D deficiency in winter, it is essential that they have access to UV information throughout the year, throughout the day, and for all sky conditions. Only satellite observations with high spatial and temporal resolutions enable the medical community to provide this public service in near real-time. The RISC-UV project was the opportunity to show that this challenge is now possible: UV monitoring can be done by using satellite products (OMI, GOME-2) and UV forecasts could be made by using modelling as long as cloud forecasts and the parametrisation of the impact of cloudiness on UV radiation are adequate..

The paragraph 6 answers the last question of referee 2 : “Would a (little) step further in improving the prediction of clouds and their effects on UV actually raise the awareness of the general public with the consequence of a more adequate behaviour under certain UV conditions? Or would an improved education (e.g. in schools, in media etc.) about the danger of UV overexposure be due to a more efficient effect? Considering/balancing these aspects in view of the achieved results could be a point in ‘Discussion’/‘Conclusions’.”

We agree that improvement of the education of the public is a key point for the future health policy about the risk and benefit on health. The improvement of the UV forecasts will not be an effort sufficient enough, educational programs will be necessary. We have

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written that in the sixth paragraph:

Paragraph 6:

Efforts are again necessary to elaborate an efficient general public health policy about risks or benefits of UV radiation. The RISC-UV project has shown that it is necessary to improve the consideration of the parameters influencing the UV radiations (aerosols, cloud covers, etc.) in the available UV climatologies. The recommendation made from these climatologies has to be easily understood by the general public. To achieve this issue, efforts will also be necessary in the education of the general public (in school, in media, etc.). In France such effort is in progress, especially in the schools via the program "Vivre avec le soleil" (Bense et al., 2009).

Summary

Following referee recommendation, 'Introduction', 'Discussion' and 'Conclusions' have been rewritten in a more concise manner.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17375, 2011.

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11, C9587–C9599, 2011

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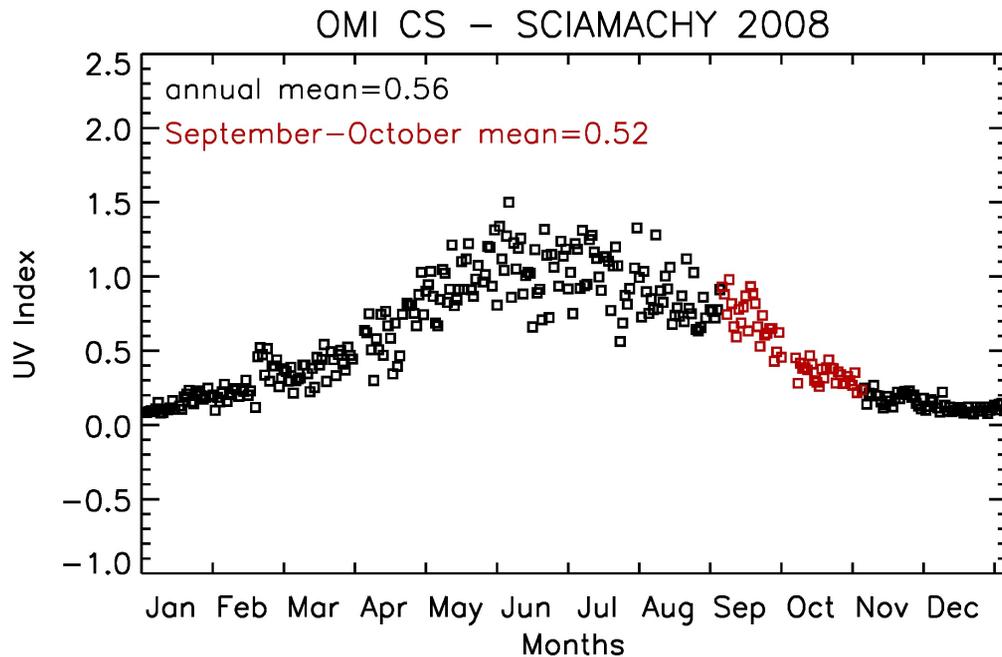


Fig. 1. new OMI clear-sky - SCIAMACHY (2008) figure

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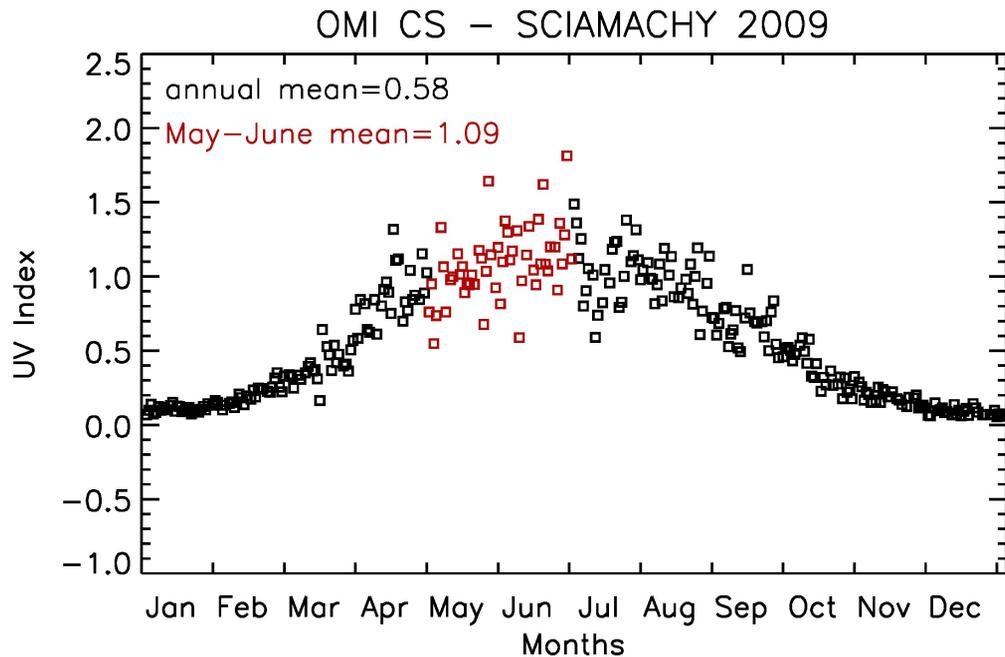


Fig. 2. new OMI clear-sky - SCIAMACHY (2009) figure

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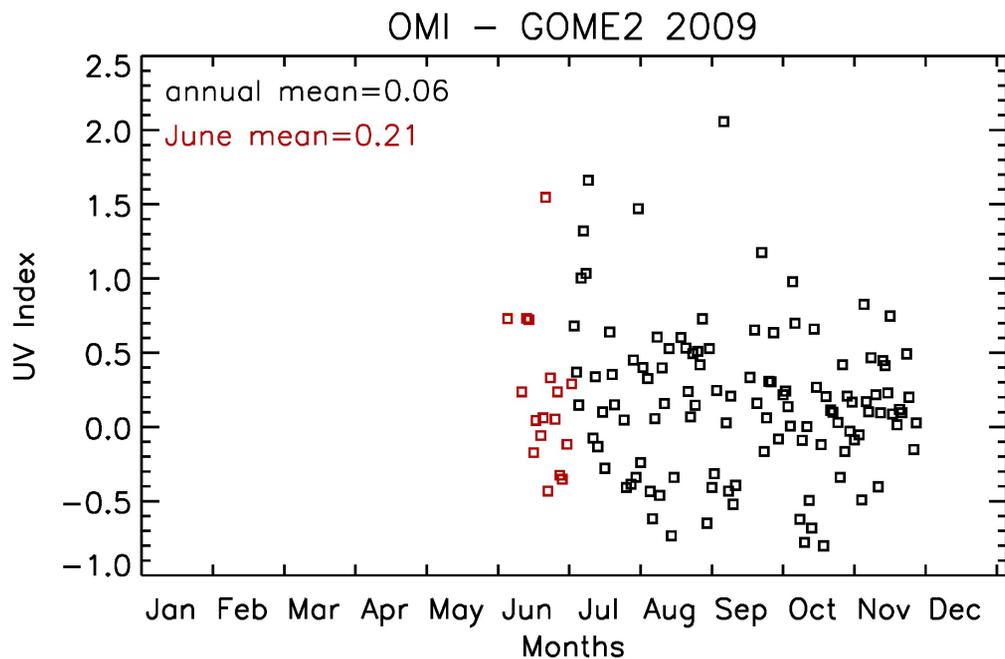
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**Fig. 3.** new OMI - GOME2 (2009) figure

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