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Interactive comment on “Comparing OMI UV index to ground-based measurements at two Finnish sites with focus on cloud-free and overcast conditions” by M. R. A. Pitkänen et al.

Anonymous Referee #3

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Review of the paper “Comparing OMI UV index to ground-based measurements at two Finnish sites with focus on cloud free and overcast conditions”, by Pitkänen et al.

This paper is focused on the validation of UVI index product given by the OMI sensor. It is well established by the scientific community (previous published works) that an over-estimation of this product is obtained when compared with the UVI index obtained by ground-based data. This bias is clearly affected by clouds and the paper deals with this point as main element, trying to get information about the cause of this positive bias, based on the cloud classification. The results indicate that no satisfactory progress has been obtained despite the effort in cloud classification. Although the errors (table

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Interactive Discussion

Discussion Paper



Interactive
Comment

I) under overcast conditions seem to be very high, they are very reasonable given the low OMI values. It seems that the retrieval under overcast conditions is in the limit of the instrument and algorithm performance. It is unclear the need of very accurate UVI estimation when UVI is very low (below 4), because in these conditions it has little incidence on human life and plants.

The title appears to be not adequate, because it seems that broken clouds are not considered and this is not the case.

In the abstract the authors say: “Satellite based surface UV product of the Ozone Monitoring Instrument OMI was validated using ground based UV measurements from the two Finnish sites Jokioinen and Sodankylä”.

The authors try to validate. Better say “compared in order to validate” or something similar. Furthermore, it is relevant to mention in the abstract what type of ground-based instrument has been used (broadband-radiometer, etc.). In this case the SL501 must be mentioned.

Introduction: In general, it seems that higher positive OMI bias is observed when broadband radiometers are used, as compared to Brewer spectro-radiometers. Can this sentence be investigated (confirmed or not) in the introduction based on the existing bibliography? Anyway, this point must be discussed in the introduction. Certainly, the OMI bias is very dependent on the measurement site. Can the authors establish what are the known results in these two sites related to validation of OMI UVI? It seems that there are earlier papers about this validation in this area of Finland.

Section 2. It seems necessary to know in more detail how the SL501 radiometer was calibrated, apart from the comparison with the Brewer instrument. Did you use a calibration constant or a table of calibration depending on SZA and ozone amount? This point is relevant in order to justify the new calibration method proposed later in this paper.

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Discussion Paper



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Comment

Section 2.1. The clouds in OMI algorithm are embedded in a scattering molecular atmosphere with ozone absorption and, while tauc is considered a spectrally invariant property of the cloud layer, CMF is wavelength dependent due to molecular scattering and ozone absorption (see for example Lindfors and Arola, 2008).

This point is also analysed afterwards. Can the authors explain in more detail why CMF is wavelength dependent and the CMF(LER) is not? This is not so evident for most of the readers. Section 2.2.1. The authors have written: “To obtain UV index from Brewer measurements for the purposes presented in the next section, full irradiance spectra in the erythemal range 290–400 nm are needed. This was done using SHICrvm software package that combines the measured spectra with an adjusted extraterrestrial solar spectrum to obtain a standardized irradiance over the erythemal range, similarly as in Tanskanen et al. (2007). The software is available and documented at www.rivm.nl/SHICrvm.” This is related with the fact that the authors work indistinctly with different wavelength intervals: OMI (erythemal range), SL501 (erythemal range), BREWER (290–320 nm). The problem of extending the Brewer spectral range up to 400 nm is not adequately explained here. Even though it may be related with the SHICrvm algorithm, this is not really the subject. This point is solved in Tanskanen et al (2007) but it is not well explained here.

Section 2.2.2 the authors have written: “To achieve a higher data accuracy required for the evaluation of OMI products, an additional SL501 calibration was done near each OMI overpass”. This sentence is due to the fact that calibration of SL501 depends on the SZA. Please indicate the range of SZA at OMI overpass at the two sites.

What do the authors indicate with the sub-index OP (overpass?)

This reviewer is not sure that the procedure to evaluate the new calibration of SL501 is certainly meaningful. As mentioned above, the cause of proposing a new calibration is not clearly established in the paper and it must be justify. Is it due to the high dependence on SZA of the calibration constant of SL501 radiometer?. On the other

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Interactive Discussion

Discussion Paper



hand, to calibrate the instrument just in the time of OMI overpass sounds a bit cheating.

If the two above major questions are justified, to say that R_{op} is constant during 30 minutes brings the reader to confusion. One time the calibration factor is obtained, it is valid “forever”. Hence, as indicate above, the authors should analyse this dependence on the SZA over the constant R_{op} .

In my opinion the advantage of broadband radiometers compared with Brewer is their high temporal resolution (they can follow cloud variations). But it gets lost because of the calibration issues.

Does all this long procedure and enormous effort for cloud classification make sense, when finally the data to be validated are time-averaged around the overpass? Broken clouds can modify the sky structure in only one minute. As mentioned above, it is not the case if we are in the limit for improving instrument and algorithms under broken clouds and overcast conditions.

Minor grammatical errors detected

1. Some words like “to” or “the” are missing in the text.
2. Pag 492, line 7. It seems that the parenthesis must include the reference of Krotkov and not only the year.
3. In page 489, line 28 , “specific” by “specific”
4. Pag 500 line 4, correct “significant”
5. In page 504, line 12, change “overpass” for “overcast”

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Discussion Paper

