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Atmospheric Measurement Techniques
Discussions

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.

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Thank you for the extensive comments on our AMTD publication. We answer to your comments below. Please note, that the OMI UV index data used in this revised AMTD publication has been corrected for absorbing aerosols following the recommendation from Referee #2. This changes the results slightly, but does not induce changes in our conclusions.

Referee comment: "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 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."

We agree, that the adverse effects of solar UV radiation on plants and humans decrease with UV index. However, from the point of view of developing UV algorithms it is crucial to know the conditions where they work well and where there is still room for improvements. To our knowledge, cloudiness classification has not been done in earlier OMI to ground based UV index comparisons from both ground based and satellite based approaches in the same study, and with large samples in overcast conditions.

It is presented here in our paper, and while the results seem to be similar to what has been seen in earlier studies, it does not, in our view, mean that no progress had been made. Some papers (referred to in the introduction) show indications, that OMI UVI might be overestimated in overcast conditions, but they show no evidence, that the entire OMI footprint had been cloud covered when the ground station saw an overcast sky. Thus, OMI UVI overestimation could have been contributed by large clear sky areas seen by OMI, but not by the ground based instruments. In our study, however, we make efforts to confirm, that both ground based and satellite fields of view were indeed fully covered by clouds, and this one of the most impostant new aspects we offer. We make efforts to bring this message through more efficiently in the revised version of the paper.

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

The title has been changed to help readers avoid the misconception, that broken cloudiness would have been excluded from the analysis. However, the focus of the AMTD paper still is on clear sky and overcast situation, given that 1) the ground based reference UV index has problems in broken sky cases and 2) our main conclusion are drawn from the analysis considering clear sky and broken sky situations.

Referee comment: "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."

We agree on this. The word "validated" was rephrased and SL 501 is brought up in the abstract.

Referee comment: "Introduction: In general, it seems that higher positive OMI bias is observed when broad- band radiometers are used, as compared to Brewer spectroradiometers. 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."

The purpose of this work is to investigate why different kinds of cloudiness seems to cause an onverestimation by OMI UV index, with extra care taken in order to confirm the type cloudiness conditions. We wish to utilise the best possible ground based reference data for comparison with OMI UV by combining the best sides of spectroradiometers and broadband radiometers that we have available. Thus, in our view it seems not

so relevant to confirm, in the scope of this paper, which instrument type may show a higher OMI bias in general. If there are papers published on OMI overpass UV index validation at Jokioinen and Sodankylä sites, the matter certainly needs to be cited in our work.

Referee comment: "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 cali- bration 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."

Thank you for this good point. The SL 501 instruments used in our setup were calibrated with a calibration constant, which applies for all conditions with some uncertainty. The method cannot, of course, take into account the possible uncertainty dependencies on solar zenith angle and ozone for instance. Instead we calibrate SL 501 using Brewer data to overcome this issue in the conditions close to OMI overpass, as presented in section 2.2.2.

Referee 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."

The reasons for the difference in wavelength the dependency of CMF and CMF_LER is now briefly mentioned in section 3.2, after the definition of CMF_LER.

Referee comment: "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 spec- tra in the erythemal range 290–400 nm are needed. This was done using

SHICrivm software package that combines the measured spectra with an adjusted extraterrestial solar spectrum to obtain a standardized irradiance over the erythemal range, simi- larly as in Tanskanen et al. (2007). The software is available and documented at www.rivm.nl/SHICrivm." 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 SHI- Crivm algorithm, this is not really the subject. This point is solved in Tanskanen et al (2007) but it is not well explained here."

The description of the SHICrivm techniques was indeed unclear. While the very detailed explanation of the tool still remains outside the scope of this article, we edited this part of the text to be more straightforward. The SHICrivm tool provides a commonly used and well tested (see http://cordis.europa.eu/result/rcn/32047_en.html, for instance) method for deconvoluting measured UV spectra and calculating weighted irradiances from the deconvoluted spectra.

Referee comment: "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."

The solar zenith angle range is now mentioned in section 2.3. However, we wish point out, that SL 501 calibration with Brewer was done not only to account for changes in SZA, but also to minimize other uncertainties, such as changes in ozone.

Referee comment: "What do the authors indicate with the sub-index OP (overpass?)"

Correct, OP stands for overpass.

Referee comment: "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 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 Rop is constant during 30 minutes brings the reader to confusion. One time the calibration factor is obtained, it is valid "forever".

Principally, the SL 501 radiometers used in this study were calibrated with a single calibration constant, which cannot account for cosine correction and nonideal wavelength response of the instrument other than in the conditions during the calibration. So, we calibrate SL 501 as close to Aura overpass as possible, so the conditions dont change much between the calibration and satellite overpass. We reformulated the Section 2.2.2 to be more consistent with the common calibration nomenclature related to radiometers, so that the reasoning would be easier to understand for the readers.

Referee comment: "Hence, as indicate above, the authors should analyse this dependence on the SZA over the constant Rop."

In our analysis, SZA does not change very much between the calibration of SL 501 and the satellite, because we required, that the calibration was done close the the time of the overpass. The importance of this issue is discussed also in the revised section 2.2.2 in more detail.

Referee comment: "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".

We agree, that the high temporal resolution is the advantage of SL 501 for this particular purpose, and that is the reason why we chose SL 501 as the ground based reference. However, we disagree, that the advantage would be lost in the calibration. The calibration sets the SL 501 UV index to the same level as Brewer UV index, and the cloud variations are still captured by SL 501 when SL 501 UVI is averaged over the +-30 min time window near Aura overpass.

Referee comment: "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."

The purpose of the 60 minute averaging of SL 501 UVI is to capture the changes in surface UV radiation caused by the variations in clouds. This is explained in the third paragraph of section 2.3 as well as in Fig. 4 and in the discussion related to it. On the other hand, the purpose of the cloudiness classification is to ensure that cloudiness type does not change (remains either clear sky or overcast) during the SL 501 UVI averaging. This is desirable, so that 1) SL 501 calibration is useful and that 2) the OMI-SL501 UVI comparison would not suffer from broken cloudiness (please see Fig. 5 and the discussion related to it) neither from the ground based point of view nor from the satellite point of view (which was estimated by using the MODIS cloudiness data).

One of the conclusions of this work is, that when the sky seen from the ground based instrument is fully covered in clouds, OMI tends to overestimate surface UV index and the bias is not likely explained by broken cloudiness in the field of view of OMI. This is an important point, when developing and further improving satellite UV algorithms.

Referee comment: "Minor grammatical errors detected"

We improved the grammar of the text.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 487, 2015.