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 your insightful comments. Our answers to your specific comments are below (page and line number refer to the corresponding comment made by the referee):

p. 488, lines 13-14: "The sentence is not clear, 21 % is for the average of the 2 sites, while 56% is only for Jokioinen."

The sentence was clarified to indicate where the averages come from.

p. 491, line 10: "The authors should state that the cloud corrected UV index UVIomi corresponds to overpass (UVI is also available at solar noon)."

We added, that it is the overpass UVI that was used.

p 491, line 13-14: "I don't understand the sentence 'Additionally, UVIomi,cs was used to calculate CMF=UVomi/UVomi,cs ...'. Indeed, in the following the authors describe the OMI algorithm (according to Tanskanen et al, 2006) where it is stated (lines 25. . ., and p. 492, lines 1-4) that a cloud optical thickness is determined and THEN the CMF is derived, and finally UVomi is computed. The authors must reconsider the order of the various steps in the algorithm. Their explanations are very confusing."

We agree, that the concept of CMF in our work could have been explained in a more clear way. The point was, that CMF is not available in the operational OMUVB products, thus we needed to obtain it afterwards. This was done by running the OMUVB algorithm by ourselves and collecting the overpass clear sky UV index from the data stream and finally, that was used to calculate CMF_UVI. The section 2.1 was reorganized to clarify this idea.

p. 492, lines 22-23: "In the present study the authors use the old version of OMI products where the aerosol absorption is not accounted for. They give an order of the correction factors that are not negligible especially in Jokioinen, leading to about 5-10% difference. So, I think it would be better if they used the new version, available since March 2014 in the AVDC website. It would thus also avoid the comment in p.502, lines 10-13. I strongly recommend reprocessing the comparisons with the new data."

Thank you for the comment, we agree with you on this matter. We have now applied the UV index correction for absorbing aerosols. Please see the effects of the correction in the revised paper Fig. 3, Fig. 4, Fig. 6, Fig. 7, Fig. 8 and Table 1 and in the related discussion. In short, the overestimation by OMI UVI is now smaller than without the correction, which is expected due to aerosols attenuating UV radiation. Yet, the positive UV bias in overcast situations remains notably larger than in clear sky conditions and so, our main conclusions remain unchanged.

p. 493, lines 11-13: "What do the words 'combines', 'adjusted' and 'standardized'

mean in 'combines the measured spectra with an adjusted extraterrestrial spectrum to obtain a standardized irradiance' ? The authors must explain clearly what the SHICrivm software makes (not only 'combines...')."

The description of the SHICrivm techniques was indeed not unambiguous. While the very detailed explanation of the tool still remains outside the scope of this article, we made this part of the text more straightforward. The SHICrivm tool provides a commonly used (see http://cordis.europa.eu/result/rcn/32047_en.html, for instance) method for deconvoluting measured UV spectra and calculating weighted irradiances.

p 495, lines 26-27: "The threshold of irradiance between uncovered and cloud covered solar disc is set to 120 Wm-2. How is determined this value? Is it independent on solar zenith angle, i.e. on site location and time?"

120 W/m2 is the commonly used and recommended threshold limit for sunshine recorders to separate cloudy and cloud free conditions since the 1980s. Please see WMO Guide to Meteorological Instruments and Methods of Observation WMO-No. 8 (2008 edition, Updated in 2010), page I.8-1 for more details. A reference to this WMO document is made in the revised AMTD article, however further analysis of the recommendation is out of the scope of this study.

p 498: "The statistics are performed with quantities that seems rather strange to me: a/ I agree with the definition of the bias MB, but I am surprised with that of the relative mean bias rMB. Maybe it should be better to define the 'mean relative bias', the denominator should not be an average, and it should be inside the parentheses within the summation. Thus, the new rMB would be what the authors call after rE. b/ What is called 'relative error' is badly named : it is a mean relative DIFFERENCE, so in fact it is the bias, rMB (the new one, see above). c/ I agree with the definition of the RMSD, but again not with the rRMSD definition. As for the rMB the denominator should not be an average, and it should be inside the parentheses (and therefore squared) within the summation. The authors must use the correct definitions. I am afraid that the results

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will be completely different."

Based on this comment, we see two things to improve in our article: 1) reasoning the choices of our statistical UV measures and 2) their nomenclature.

Concerning 1), we think it is insightful to present rBM in addition to rE, because rMB describes the mangitude of MB in relation to the average UV index of the sample (eg. clear sky, or overcast). The perhaps more commonly used rE on the otherhand, gives partly the same information, but it gives much weight on the observations of near-zero SL 501 UV index (because it is the denominator). The same reasoning applies for rRMSD, which describes the RMSD in relation to the average UV index of the sample. Nevertheless, we wish to present rE as well, like you suggest, because some readers might be more familiar with it than with rMB.

Concerning 2), we kept the name of rMB, and mentioned the meaning explicitly in the article. Also, we renamed rE to rD after relative difference. Further, we renamed rRMSD to CV (coefficient of variation in RMSD), which is how the same statistical measure is also known. After all, when all the statistics are explicitly defined in our text, readers should be able to avoid misunderstandings related to our statistical definitions.

p 499 line 12: "The authors say 'Kazadzis et al. also concluded that a longer averaging time window...'. The time window should be 'longer' than what? Please, complete."

The sentence was clarified to correspond better to the original paper. They also showed, that a time window of 60 min is suitable for this purpose.

p 502, lines 10-13: "The correction for absorbing aerosols is given only approximately. As I told in the comment above, for p. 492, this part could be removed if the authors were making use of the new OMI dataset. And thus the remaining bias would be attributable only to cloudiness (assuming of course that the previous aerosol correction is correct)."

As mentioned for the comment p. 492, lines 22-23, we have now applied the correction

for absorbing aerosols.

p 502 lines 28-29: "the scatter SD(diff) is about 0.40UVI for broken sky situations (according to Table 1), that does not sound similar to 0.22UVI and 0.13UVI. Please explain/reconsider your comment."

The unclear statement was removed to avoid confusion.

Technical corrections: All technical corrections accounted for, except the comment on the typo on p 495 line 5, which for some reason does not appear in our source file. Also the corrections for p497 line 12 and Fig 3 top panel were not done, since the correct unit for sunshine duration here is indeed [s/min], as these are one minute values instead of hourly values. Further, Fig. 5 is intended to clarify the statistical FOV bias on a conceptual level, and no numerical data has been used to scale the y-axis exactly. This is why we wish to avoid placing numerical ticks there. In order to clarify Fig. 5, we have added more explanation on it in Section 3.1, as suggested by Referee #1.