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Atmospheric Measurement Techniques Discussions

Interactive comment on "A new method for estimating UV fluxes at ground level in cloud-free conditions" *by* William Wandji Nyamsi et al.

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First of all, we thank Referee #3 for these positive remarks on this topic. The authors believe that they have understood the concerns of the referee. Their remarks have been taken into account for revising a part of the text following recommendations of the referee.

Comment 1. Page1, lines 29-31: It is claimed that improvements in the modelling of the Earth's reflectivity in the UV region are necessary. However, I cannot see that the manuscript identifies high reflectivity to be a problem when discussing the model versus measurement results in Figs.2-6 and 8 or elsewhere in the manuscript. Some weak hints are given, but no strong evidence supports the claim in the abstract. It should

be possible to identify measurements made during high and low albedo situations and compare these with the model to quantify differences caused by differences in the Earth's reflectivity.

Answer: Thank you for this valuable remark. We fully agree with you. It is also a comment from the referee #1, hence the answer is the same. We have done more investigations to explain these underestimations. We have found that it is related to albedo values. We have added a paragraph in the text to better clarify these observed underestimations in the Figure at the second paragraph of section 4.1 as follows: "Even if the points follows quite well the perfect line (Figure 2a), a set of points is seen where the method underestimate noticeably by more than 20%. These underestimations occurs between ending May and mid-July. During that period, the shortwave albedo was less than the effective UV albedo by a factor 0.8. The effective UV albedo is part of the Version 2 dataset and was derived by comparing measured clear sky spectra with corresponding radiative transfer model results (Bernhard et al., 2007). As a smaller albedo means a smaller contribution to the diffuse part of the irradiance, the difference between the shortwave and effective UV albedo may explain these underestimations seen in Figure 2a"

Comment 2. Page 3, line 25: "A very large underestimation" is mentioned. To make the manuscript complete and self-contained: may you please include numbers quantifying this underestimate?

Answer: Thank you for this remark. We have included numbers quantifying this underestimation. We have written the sentence as follows: "On the contrary, a very large underestimation of the transmissivity was observed in KB3 [283, 307] nm and KB4 [307, 328] nm by respectively 93% and 16% in relative value and exhibits relative root mean square error of 123% and 17% in clear-sky conditions. Similar relative errors are observed for cloudy conditions."

Comment 3. Page 3, lines 29-30: Please include numbers quantifying how much the

"novel parameterization" improved the transmissivity.

Answer: Thank you for this remark. We have included numbers quantifying this underestimation. We have added the sentence as follows: "The novel parameterization of the transmissivity using more quadrature points yields maximum error of respectively 0.0006 and 0.0143 for intervals KB3 and KB4."

Comment 4. Page 6, lines 4-5: If I understand this correctly, the SHICrivm software is used to obtain the full UV spectrum from the measurements. The Sodankylä Brewer stops at 325 nm while the Jokionen Brewer stops at 365 nm. Thus, for the Sodankylä Brewer nearly the whole UV-A is extrapolated from the UV-B using SHI-Crivm. This approach should be justified and the errors in the extrapolated part assessed and documented. For the Jokionen Brewer the extrapolation is less severe, but needs nevertheless to be justified and the uncertainty discussed.

Answer: Thank you for this remark. We fully agree with you on the importance of inclusion of a discussion on the uncertainty introduced by the UVA extension of the measured spectra. We may estimate the approximate uncertainty at least for the UVA doses as follows. Mäkelä et al. (2016) ended up with uncertainties as high as approx. 2% caused by the constant scaled UVA extension in non-weighted UVA doses. The method investigated therein is part of the routine processing scheme used in handling the UV irradiance spectra measured with the Brewer spectroradiometers of the FMI. The method used by ShicRIVM to extend the spectrum beyond the upper limit of the measured wavelength range uses the same kind of scaling. ShicRIVM also involves use of an atmospheric transmission model that takes into account the diurnal/seasonal/climatological variations in the shape of the spectrum. From the spectra measured during an intercomparison in 2000, the UVA for the spectra that were cut off (and extrapolated using ShicRIVM) were compared with the full spectral analysis with following results: The daily average UVA ratio of the extrapolated 325 to 400 nm was: 1.028, where for individual spectra the relative standard deviation was 6.2%. For the 365 cut off extrapolation the ratio was 1.014 with the relative standard deviation 1.4%

(Slaper, 2017, personal communication). The time window was on daily scale. Since we are dealing with instantaneous measurements, the uncertainties are estimated to be somewhat higher. We have re-written this part of the text in the manuscript accordingly.

Comment 5. Page 6, line 8: Please do not use tinyurl and elsewhere. It is just obfuscating.

Answer: Thank you for this remark. We fully agree with you. Done as requested.

Comment 6. Page 6, lines15-16: The sentence "If there is no cloud, the sky should be clear and steady for a long period" is pretty obvious and may be omitted. But maybe you intended to say something else?

Answer: Thank you for this remark. We have re-written this part of the text in the manuscript.

Comment 7. Page 7, line 5: Please be a little more specific than "one of several available databases" and mention which one you used, including references and/or URLs.

Answer: Thank you for this remark. We fully agree with you. We have re-written the sentence to make it clear and add the url as follows: "Ground elevation is extracted from SRTM (Shuttle Radar Topography Mission) database and has been downloaded from the website http://srtm.csi.cgiar.org/SELECTION/inputCoord.asp"

Comment 8. Page 7, lines 20-25: You seem to be concerned that the albedo changes when approaching the NIR. But that should be of no relevance for the work presented here as you only discuss UV-B and UV-A. Hence, the discussion about NIR albedo may be omitted.

Answer: Thank you for this valuable remark. It is also a comment from the reviewer #2, hence the answer is the same. We have re-written this part of the text to make it clearer as follows: "As a first approximation, the UV albedo is assumed to be spectrally

constant and equal to the shortwave albedo. This assumption may result in biases depending on the surface. For example, in the case of snow surface, Varotsos et al. (2014) reported from many aircraft measurements that spectral albedo exhibits a tendency to decrease with increasing wavelength, about 0.7 from UV to about 0.4 in the NIR independently of the sky conditions. Therefore, the albedo integrated over the spectrum, becomes less than 0.7 resulting in underestimation in UV albedo, hence in a lesser contribution to diffuse UV irradiance and therefore to underestimation of the global UV."

Comment 9. Page 9, line 22: Nine variables are used to build the atmospheric states. One is the solar zenith angle which is sampled uniformly between 0 and 89. It should be noted that the solar zenith angle dependence of the radiative transfer equation is best described by the cosine of the solar zenith angle and not the solar zenith angle. Hence, in your table 2 it would have been preferable to have cos(θs) instead of the θs.

Answer: Thank you for this remark. Done as requested.

Comment 10. Page 10, line 8: What is meant by "excepted for KB5"?.

Answer: Thank you very much for this remark. It was a typing error. We have corrected.

Comment 11. Page 13, lines 22-23: I do not comprehend the sentence "Further investigation reveals a systematic overestimation at the low irradiance from the method itself". What is this systematic overestimation of the method itself? Is there a problem with the method? Why has not that problem been corrected?.

Answer: Thank you for this remark. We fully agree with you. We have re-written this part of the text as follows: "For the wavelength lower than 320 nm, in Figure 1, the proposed method seems to mostly overestimate when compared to the detailed spectral calculations serving as reference. This observation induces a systematic overestimation at the low irradiance from the method."

Comment 12. Page 22, Table 2: The solar zenith angle is sampled uniformly between

0 and 89. Was your radiative transfer calculations done in plane-parallel or pseudo-spherical geometry? Please include this information in the model description part.

Answer: Thank you for this remark. We fully agree with you. The radiative transfer calculations was done in plane-parallel geometry. We have included this information in the first paragraph of the section 3.

Comment 13. Page 22, Table 2: Include a column that for each variable gives the total number of samples for each variable (for Aerosol type that is obviously 7, for many of the others it is not possible to tell from the table as is). Also, where applicable, include steps. That is, for uniform distributions you include start and stop, but should also include step size.

Answer: Thank you for this remark. A clear-sky atmosphere is a combination of variables. Therefore, the number of clear-sky atmosphere is the number of samples of each variable. The number of samples depends on what we need to do with. Every time in the text, we have mentioned the number of clear-sky atmosphere which is also equal to the number of samples used for each variable.

Comment 14. Page24, Table 4: Why is the rBias so much worse for the direct than the global irradiance? Is this due to a worse sampling as in Fig.1 for the direct irradiance? The global irradiance includes the direct irradiance. Is thus the error in the global irradiance mostly due to the error in the direct irradiance?

Answer: Thank you for this remark. The errors observed in UV-B direct irradiance are due to the sampling. By increasing the number of NBk precisely in KTKB3 due to the strong ozone absorption in this band, the resampling technique may provide better results. In the proposed method, we have selected a single NBk over the band for the linear interpolation. As result, that produces an overestimation on this part of the UV spectrum. In addition, the UV direct irradiance intensities are extremely low and may produce high relative values.

Comment 15. Pages 25-26, Tables 5-6: Please include the number of data points included in the analysis for each station. This is valuable information to be able to better assess the numbers in the tables, as a station with more data points maybe considered more “valuable” than one with fewer.

Answer: Thank you for this remark. Done as requested.

Comment 16. Page 27, Fig. 1: Please indicate (label) where the various KTKB3, KTKB4, KTKB5, and KTKB6 bands are on the green line

Answer: Thank you for this remark. Done as requested.

Comment 17. Pages 27-32, Figs. 2-6: Please combine these Figures into one as you have already done in Figure 8. Figure 8 is much easier to read and allows for much easier comparison of results from the different stations than Figs.2-6.

Answer: Thank you for this remark. Done as requested.

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