Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-223-AC3, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

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.

William Wandji Nyamsi et al.

william.wandji@fmi.fi

Received and published: 3 November 2017

First of all, we thank Referee #2 for the positive remarks on this article. The authors believe that they have understood the concerns of the referee. The remarks have been taken into account for revising a part of the text following recommendations of the referee.

Major comments

Comment 1. Page 6, lines 9-16: the proposed method (Lefevre et al., 2013) for the definition of clear skies is applied on broadband or total irradiance. Is this valid for UV radiation as well? UV radiation is affected considerably more by scattered cloudiness. In this case, you may have an unobstructed Sun (no clouds to cover) and a

Printer-friendly version

Discussion paper

non-significant effect on diffuse broadband irradiance, so, you can assume that you have a cloud free instant. In UV (direct and diffuse irradiances) however, the effect of scattered cloudiness will be more evident. This is one of the cases that the cloud modification factors in UV and broadband irradiance are not related with a linear fit. Can you provide some evidence that the propose method is valid for UV as well?

Answer: Thank you very much for this remark. We fully agree with you. The proposed method of Lefevre et al., (2013) for selecting clear-sky instants uses broadband irradiance. Since we have these kind of measurements for both Finnish stations, we are able to apply the Lefevre et al. (2013) method. We have assumed that if a clear-sky instant detected with broadband irradiance, is also clear-sky instant for any spectral measurements. We have re-written a part of the paragraph as follows: "We assume that a cloud-free instant detected by analyzing broadband irradiances is also cloud-free for the spectral measurements. It is possible that UV is affected by the presence of scattered cloudiness which may go unnoticed in the broadband range and that the retained series of cloud-free instants for broadband may comprise cloudy instants for UV. Given the high selectivity of the algorithm of Lefèvre et al. (2013), we believe that such cases are rare and that the conclusions will be unaffected as a whole."

Comment 2. Page 7, lines 15-25: it is not clear in the document the type of albedo used as well as if the spectral dependency of albedo is taken into account.

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

CC BY

underestimation of the global UV."

Comment 3. Page13: Figures 7 and 9 should be discussed in much more detail.

Answer: Thank for this remark. We fully agree with you. We have provided new plots and re-written the relevant part of the text.

Comment 4. Figure 1 and relevant text: it seems that the proposed method works significantly better than the Kato et al. approach but it is not adequate for spectral irradiance calculations with e.g. 1 nm step and resolution below 340 nm. This should be highlighted in the text.

Answer: Thank you for this remark. We fully agree with you. We have highlighted it in there-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 5. Figures 2-6 and relevant text: First, the meaning of counts (colorscale) is not clear. The word "count" does not appear in the text. Second, the comparison of estimated vs measured irradiance is vastly dominated by the solar zenith angle, so such types of figures are always looking good. The authors are encouraged to present their results as differences (percentage, ratio, etc) between estimated and measured values vs solar zenith angle (like figure 9). The may skip some figures or replace with new ones. Moreover, due to the assumptions about the surface albedo, the differences as a function of season or some kind of snow measurement will be very helpful, since snow reports are kept at the selected sites.

Answer: Thank you for this remark. We fully agree with you. For the first part of the comment, we have changed the caption of the Figure as well as the relevant text as follows: "Scatter density plot between measurements of UV–A and estimates for each station with each station name at top. The colorbar indicates the number of points

CC BY

in the area within the interval 0.4 W/m2 x 0.4 W/m2" Then, for the second part, we have replaced the plots by the new ones. They are the ratio and difference. We have re-written the relevant part of the text.

Minor comments:

Comment 1. Please explain abbreviations (UV, FWHM etc). In some places, the UV radiation across the whole UV spectrum is mentioned as total or total UV. Please use just UV (280-400 nm) and UV-B, UV-A. The same stands for shortwave irradiance: it is referred as total, broadband etc. Please use one definition name

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

Comment 2. Page 2, lines 10-15: It would be better to talk about risks and beneïňĄts from UV exposure instead of talking about"healthy"sun exposure (it is actually safe exposure). Please split and present clearly the impacts from UV over-and under-exposure (related to vitamin D deficiency).

Answer: Thank you for this remark. We fully agree with you. We have replaced the word healthy by "safe". Then we have clearly presented the impacts from UV over-and under-exposure in the second sentence of the first paragraph of the introduction as follows: "For instance, UV radiation is a principal source of vitamin-D, while the excess UV exposure is a risk factor for skin cancers, cataracts and immunosuppression"

Comment 3. Page 5, line 4: please replace "fields of cultures "with" field of agriculture"

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

Comment 4. Page 5, lines 11-12: please rephrase, too many "between"

Answer: We fully agree with this remark. We rephrased. The sentence is now as follows: "the effective UV albedo varies between 0.6 and 0.95 occurring from August until November"

Comment 5. Page 6, lines 22-25: UV irradiance, especially at lower wavelengths and

CC BY

under low solar zenith angles (a usual case for high latitude stations) depends FROM the ozone vertical profile, too.

Answer: Thank you for this remark. We fully agree with you and we have added this dependence in the text.

Comment 6. Page 7, line 2: Insert world exponent: Angstrom exponent coefficient.

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

Comment 7. Page7, line 6: upwelling to downwelling flux::: add phrase “at the surface”

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

Comment 8. Table1: Brewer instruments are mentioned as spectrophotometers and SUV-1000 instruments as sperctroradiometers. Is there such a difference?

Answer: The Brewer is a spectroradiometer. Its name given by the manufacturer, however, is Brewer spectrophotometer (http://www.kippzonen.com/Product/50/Brewer-MkIII-Spectrophotometer#.WfBIhXZLeyp). We fully agree with you. We have changed the text accordingly in the Table 1.

Comment 9. Table 2 and relevant text: please add some more details about the model runs. What is the number of streams used? What about the Delta-Eddington approximation?

Answer: Thank you for this remark. We provided more details in the text. We added one sentence at the first paragraph in the section 3 as follows: “For all the radiative transfer simulations, a plane-parallel atmosphere was assumed and the DISORT 2.0 (discrete ordinate technique) algorithm (Stamnes et al., 2000) with 16 streams was selected to solve the radiative transfer equation because several articles have demonstrated the accuracy of its results when compared to robust and more time consuming solvers.”

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-223, 2017.

CC BY