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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correcting spaceborne reflectivity measurements for application in solar ultraviolet radiation levels calculations at ground level" *by* P. N. den Outer et al.

P. N. den Outer et al.

peter.den.outer@rivm.nl

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Reply to referee 1

We thank the referee for carefully reading the manuscript and supplying us with detailed comments. We believe that in dealing with the objections put forward by the referee, the clarity of the manuscript will be greatly improved. We will handle the objections successively. We copied part of each text block for identification purposes and wrote our reply directly underneath it.

"About the Figures 3 and 4. You wrote that "we expect to observe linear relationships". Why should they be linear?"

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Answer:

We expect a linear relationship with slope 1 and through the origin, because we plot the Cloud Modification Factors (CMFs) derived from spaceborne instruments versus CMFs derived from ground-based data. Ideally, they should be identical, and hence our stated expectations.

" You also wrote 'Linear relationship is indeed revealed in the EPTOMS and NIMBUS plots'. .... What I missed, however, was a discussion about what you believe you have to correct for: ...CMF for surface UV estimates."

Answer:

The discussion on this subject will be more elaborated. First of all we will explain more the general concept of using fast, i.e. straight forward cloud effect proxies in the work of UV-radiation. Our final goal is to cover the full chain of adverse and positive health effects of UV-irradiation in relation to ozone depletion and counter measures taken. Hence, what we have to correct is more empirically driven. In doing so, we touch upon the subject of spatial versus temporal averaging, where we show that averaging the cloud reflection over an area of 1x1 degrees leads to optimal agreement with UV measurements averaged over a day (i.e. time integrated). This has not been shown previously. Although discussion on page 73 is meant to be more descriptive, the message should have been that we see differences between the TOMS instruments derived products on the one hand and the OMI instrument derived product on the other hand while the terming of the data product by the supplier (NASA) is the same.

"During days of broken cloudiness, could one get on average Fgb>Fsat ..., which effect therefore should be most pronounced and visible around Fsat approx. 0.5? ...please explain the reasons in the text, and particularly the likely reasons why they now are not linear."

Answer:

We thank the referee for this interesting observation. Indeed we see that there are far more data points for which holds Fgb»Fsat than the opposite Fgb«Fsat. Generally, the curvature in these type of correlation plots is caused by the interplay between absorption and scattering combined with the effective areas compared and the action spectrum under study. The likely reason for OMI not to show a linear relationship is not easy to grasp. First of all, since all 3 cloud products (of NIMBUS, EPTOMS, OMI) are termed 'LER', it is expected that they perform similarly. Differences exist in used wavelength and initial spatial resolution, both could lead to the observed differences. A discussion will be added and made more quantitatively.

"About the Figure 4. It would be interesting to know why OMILER is different to EP-TOMS and NIMBUS. ... Not only two and contradicting statements for OMILER pattern was given, but additionally the reader gets totally confused about the resolution of OMILER data that was used in the analysis shown in the Figure 4."

Answer:

Firstly, a statement will be added that the high resolution LER data is only used in par. 3. Secondly, we agree that it could be sorted out why the distributions for OMI and the TOMS instruments differ. However, we are pointing out the fact that they do and how and what the consequences are, or could be, from a perspective of long-term UV-radiation assessment. Indeed, the difference between spatial resolution of the detector and the grid size of the data products should have been clarified more. They are two different things. The spatial resolution of the orbital scans of OMI corresponds to a surface area of approximately 13 x 24 km and that for the TOMS instruments to 40 x 40 km initially. After 1997, 50x50 km, (http://eospso.gsfc.nasa.gov/eos_homepage/for_scientists/atbd/docs/OMI/ATBD-OMI-03.pdf) for TOMS, hence, the statement 'OMI resolution is approximately. 8 times that of the TOMS instruments'. All data is delivered on a convenient location fixed grid (level3): for TOMS on a 1.25x1.0 degrees and OMI on a 1.0 x1.0 grid, a former edition was however on a 1.25 x1.0 grid. Grid cells of 1.0x1.0 degrees correspond to an area of 110 x 70 km2 in Europe. Within the gridding process, information from several orbits is integrated to deliver the data products. How this is done has, and never will be, the content of our work. Our observation is that data products on a 1.0 x 1.0 grid have different distributions, as shown in Fig 4. This difference may find its cause in the initial higher spatial resolution by which they were recorded. Averaging the gridded product, we obtain from the data providers, over more an area larger (2x2 degree) than the grid size of the TOMS data, does not yield a comparable distribution as found for the TOMS data. The discussion along the line written here will be added in the manuscript.

"About the Figure 6. You wrote that the reason for SZA dependence ... is that there are cases when RCF=0 that are due to the erroneous assumption of snow. Did you check that this was really the actual reason? ... over snow/ice covered surface."

Answer:

This is not entirely correctly quoted. The statement is that the SCATTER of the data points for large SZA is likely due to an erroneous assignment of snow cover, and not the observed (small) SZA dependence. We give the likely reason for this scatter. We could not check whether this is really the case since we do not produce the RCF-product or have the resources and access to produce the RCF ourselves. Nor is the actual data set on snow cover used for the RCF-algorithm provided. Additionally, we do not have ground-based observations of snow cover for all eighty WRDC-stations. We do observe, however, that the subset for which holds: RCF=0 and SZA>55 degrees, the data points that scatter correspond to ground-based measurements in winter and early spring. Other reasons than cloudiness are not easily found for pyranometers to deliver values for Fgb considerable smaller than one, also considering the large number of pyranometers we used for this study. Hence, we conclude that these data points belong to actual cloudy days. Now the only assignment left is the snow cover. Only a reflection (as measured by spaceborne instrument) that is erroneously attributed to snow cover instead of to clouds will lead to the observed effect. The cited webpage {http://acdb-ext.gsfc.nasa.gov/People/Joiner/OMCLDRR_README.htm} states however "Over snow/ice, the processing quality flag bit 5 is set to 1, and the cloud fraction is assigned to 1". The number of data points with 1 in our retrieved datasets is just far too little, which makes this statement unlikely. Additionally, from a "UV-radiation protection" point of view it does not make sense to assign a '1' to RCF for snow covered surfaces.

"For instance, let's assume that in the Figure 6 ...there are clear-sky conditions at the overpass time and moreover that Fgb,.. However, Fgb ...cannot be higher (than 1), but it can be lower in many cases when the entire day was not cloud-free. Moreover, OMI overpass time is not at noon, which influences most your Fgb, so this time difference can cause a more pronounced impact during these high SZA cases. So perhaps, this effect could explain at least partly the pattern for high SZA in the Figure 6?"

Answer:

Here we do not fully agree with the objection put forward by the referee Fgb can be larger than one as can be seen in the figure 3, 6, 7. We do not present results of the overpass data sets. We did not find considerable better results when using the overpass data compared to the use of gridded data, and therefore did not discuss this version of the data. The difference is small between the solar zenith angle at time of overpass and the minimum solar zenith angle of the same day. As "SZA denotes the minimal solar zenith angle reached for the day and location of the considered data point"(P74L8), days with SZA>55 are measured either in winter or up North. Meaning that the change of the solar zenith angle is not that fast, and only for latitudes at Thessaloniki, we find considerable differences between SZA and the solar zenith angle at the time of OMI overpass. We find 2+/-2 degrees for the average difference with a maximum of 10 degrees for the latitudes at the Thessaloniki location, while for other UV sites it is within a band of -4 to 2 degrees. So this will not cause the scatter observed in the plot. More over, the corresponding LER data does not show this problem. As far as we know LER data is not corrected for snow cover.

"You considered the issue of spatial resolution .., while temporal resolution was not much discussed. Your Fsat is based on overpass conditions, while Fgb is based on diurnal UV data. I think this difference might play a role in some of the plots and would have deserved at least some discussion. "

Answer:

Here we do not fully agree with the objection put forward by the referee. We think we have discussed this subject extensively in section 3. It is exactly the interplay between the spatial averaging by the satellite at one moment, and the time-averaging at the ground-station combined with the predictability of weather that make it possible to derive daily sums of ground-level UV radiation from spaceborne observations. We wrote: 'The agreement between any satellite-derived quantity and its ground-based measured counterpart will, .., also be a function of the areas that are effectively intercompared. .., we expect an optimal agreement when the satellite pixels ..have .. Field Of View (FOV) that is representative for the clouds drifting over the ground station during "mid day".'(p70L 20), and 'UV irradiance measurements have a large contribution of scattered radiation, even on cloudless days it is around 50 %, and a large area surrounding the site, 10–30 km, is of influence. The presence of clouds in the whole hemisphere is of importance and not only the clouds at zenith or in the direction of the sun. Decreasing the FOV would lead to assigning erroneously only the overhead clouds to be of influence. (p71 L6.)' Additionally, as will be pointed out more clearly in the introduction, daily UV-sums are our building blocks for long-term UV-burden assessments. So the focus is at cloud effect proxy applicable to daily sums.

"I think the authors should have discussed also the other effects than clouds in their approach to estimate the surface UV...., which was not anticipated".

Answer:

A more elaborated introduction on the use of ancillary data will be given, and a reference to the Den Outer et al. 2010 paper where essentially the same ancillary data has

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been utilized to produce the modeled data. Indeed, the site-to-site agreement ranges from -4 to +10% as indicated by Fig 2. The overall effect of the bias is reflected in the first rows of Table 5 and 6. We did not choose to present results where we had divided out this overall bias, but it might have been a better way to draw only the attention to the performance of satellite derived CMFs. Additionally, our conclusion on the best performing correction is not only based on which approach yields the smallest bias, but also on the behavior seen in Fig. 7. (higher overestimations for smaller ground-based CMFs), a quantitative ranking will be added.

"I found the description of your "mountain ridges" approach rather unclear..... Please try to clarify."

Answer:

A more elaborated description will be given along the lines: 'We want to deduce an empirical relationship (a polynomial function) and determine if additional underlying correlations exist on for instance the SZA, location, or day of year. For that we needed a reduction of the large number of data points (380k for the NIMBUS set) for the fitting process, and a disregarding of outliers within the same process. Outliers might be erroneous measurements or origin from snow cover. Both effects should not enter in the bare correlation function that we are after. Corrections for snow cover should be made in a later stage. Fig. 3 shows the density of data points the "mountain ridge" is the line formed by the connected local maxima, shown for each instrument and for the SZA-intervals indicated in fig. 4. The locations of the local maxima are sought by moving an imaginary line with slope -1 over the density plot and recording the locations of the maxima along this line while moving. A line with slope of -1 is roughly perpendicular to the orientation of the ridge. Other slopes lead to similar locations of the maxima in the density plots.'

"It is not only that the discussion about the reasons for these corrections were missing, but the reader (who would like to understand them) is even more confused here:

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if they moreover depend on the algorithm versions, what was then the difference between the "earlier version of the LER algorithm" and the more recent one?" That is indeed a true and valid question, and we fully agree with the referee that some confusion prevails. We made a statistical analysis of delivered data products and made the observations as presented in the paper. The general version indicator '8' for the TOMS-LER data set may still have different generation versions, and may involve recalibrations. Versions/generation indicators are not a part of the delivered filenames. On http://ozoneaq.gsfc.nasa.gov announcements considering recalibrations can be found, however links or the corresponding codes in the header lines of the data files are not given. We will compare the dates of these announcements with the download dates of our data set, in order to obtain information on the different calibrations. .

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