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Interactive comment on "OMI surface UV irradiance in the continental United States: quality assessment, trend analysis, and sampling issues" *by* Huanxin Zhang et al.

Anonymous Referee #2

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The manuscript by Zhang et al. describes a comparison of erythemal dose rates (EDRs) measured by the space-borne Ozone Monitoring Instruments (OMI) and ground-based UVB1 pyranometers manufactured by Yankee Environmental Systems. The ground-based instruments are located in North America (30 sites in the U.S. plus one site in Canada) and are part of the UV-B Monitoring and Research Program (UVMRP) operated by the Colorado State University. This network is currently the largest ultraviolet (UV) radiation monitoring network operating in the U.S. and data from the network are therefore important for assessing the climatology of UV radiation in the U.S. A comprehensive comparison of the network's measurements with OMI observations has not been published to my knowledge. The subject of the paper is therefore

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relevant for Atmospheric Chemistry and Physics. Unfortunately, I have concerns that the measurements of the ground-based instruments have not been processed correctly (see General Comments below) and this issue has to be resolved before the manuscript can be considered for publication in ACP.

General Comments

The manuscript compares ground- and satellite-based EDRs at the time of the satellite overpass and local solar noon. Figure 8 shows distributions of the ratio of measurements at noon and during the satellite overpass. For OMI data (Fig. 8a), the distribution is skewed towards values larger than one. This is the distribution that I would expect because OMI passes over the U.S. in the afternoon when the solar zenith angle (SZA) is larger than at noon. For days when atmospheric conditions stay constant between noon and the overpass, UV radiation is only controlled by the SZA and is therefore larger at noon than during the time of the overpass, resulting in a distribution like that shown in Fig. 8a. In contrast, the distribution of the ratio of noon and overpass measurements for ground-based data (Fig. 8b) is almost symmetric with a mean value of about 1. I find this result very surprising and it conflicts with my understanding of the radiative transfer at Earth's surface. I therefore suspect that the ground-based measurements were not processed correctly, and if so, this would have consequence for the majority of data presented in the manuscript. All affected data would have to be reprocessed.

Over the U.S., the OMI overpass occurs approximately between 0 and 2 hours after local solar noon. Hence, the SZA at local solar noon is almost always smaller than at the time of the satellite overpass. UV radiation must therefore be larger, on average, at noon than at the time of the overpass. Of course, UV radiation at the surface during the overpass time may occasionally be larger than during noon, for example, if enhancement by scattered clouds occurs. However it is highly unlikely that atmospheric conditions (e.g. clouds, aerosols, ozone) change in a systematic way between noon and overpass, resulting, on average, in higher absorption at noon to compensate for the smaller SZA. For example, if clouds always had a larger optical depth at noon than at the overpass time, the effect could be explained. It is hardly possible that such a systematic cloud effect could occur for the majority of the 31 sites studied here. Hence, the symmetrical distribution centered at one (Fig. 8b) points to a problem in the data analysis (perhaps the calculation of the time of local solar noon is incorrect), which must be resolved before the paper can be published.

My second "major" comment refers to the selection of results being presented. The paper features an abundance of statistical analysis parameters but not the information that most readers would be most interested to see, i.e., the mean bias and range between ground-based measurements and OMI at each site. I therefore propose to include either a new table or a figure with box-whisker plots, which would show for each site the median and average relative bias, the interquartile range, and the 5th and 95th percentile range, for small SZA (e.g., 0 - 50 degree) and large SZA (50 - 75 degree), and for noon and overpass data. Such box-whisker plots would be similar to the box-whiskers shown in Figure 11, and would show data for each of the 31 sites separately, but only for two SZA ranges (which I think is sufficient). Such statistics are much more useful than Taylor diagrams, which are hard to interpret, and separating the results per site would allow to better discuss regional differences in the deviation of ground-based and OMI data, along with their causes (e.g., elevation, proximity to pollution and aerosol sources, clouds, etc.). If a figure with box-whisker plots is chosen, those could be spread-out over two or three rows to be able to show enough detail. Box-whiskers are easier to visually take-in than a table, but a table has the advantage that the numbers are defined. Finally, I don't see much value in Figures 5 and 6 and these could be removed (see details below).

The title of the paper should also mention the ground-based measurements. I propose: "Surface erythemal UV irradiance in the continental United States derived from ground-based and OMI observations: quality assessment, trend analysis, and sampling issues"

Specific comments:

P1, L17: As mentioned above, I find the conclusion in the following paragraph very surprising and physically impossible

"In addition, the ratio of EDR between solar noon to overpass time is often (95% in frequency) larger than 1 from OMI products; in contrast, this ratio from ground observation is shown to be normally distributed around 1. This contrast suggests that the current OMI surface UV algorithm would not fully represent the real atmosphere with the assumption of a constant atmospheric profile between noon and satellite overpass times."

The summary in the abstract needs to be revised once the issue of the surprising distribution of the ratio of noon and overpass ground-based data is resolved.

P1, L21: Change "Both OMI Noon_FS and ground peak EDR show a high frequency of occurrence of $\sim$ 20 mW m-2 over the period of 2005–2017. However, another high frequency of $\sim$ 200 mW m-2 occurs in OMI solar noon EDR while the ground peak values show the high frequency around 220 mW m-2, implying that the OMI solar noon time may not always represent the peak daily UV values." to: "The distributions of the OMI Noon_FS and the ground EDR were analyzed using data for the period 2005-2017. Both distributions have local maxima at about 20 mW m-2. The overall maximum of the distribution is 200 mW m-2 for the OMI and 220 mW m-2 for the ground-based data."

P2, L35: The references of the two assessment reports (UNEP, 2007; WMO, 2010) are now fairly old and should be updated by references to the latest assessment reports (UNEP 2015 and WMO 2014).

P2, L40: Bigelow et al., 1998; Sabburg et al., 2002; Levelt et al., 2006 is an odd collection of rather old papers. Perhaps some newer papers should be cited also.

L55: Explain acronyms UVMRP and USDA.

L56: The Brewer network is also still active in the U.S., see https://www.esrl.noaa.gov/gmd/grad/neubrew/ Please mention it!

L68: Also mention an example from South Americal, e.g., http://dx.doi.org/10.1016/j.jphotobiol.2012.06.013

L142: the standard definition for UV-B is 280-315 nm, not 280-320 nm.

L146-147: For satellite validation, the uncertainty for SZA > 80° is of lesser importance. What is the uncertainty for the SZA range applicable to OMI validation? (While the specification of deviations from measurements with the standard triad is interesting, the 2.8% specified here is not an uncertainty because measurements of the triad are not free from uncertainty.) Also, please specify the confidence interval of all uncertainty specifications.

L148: I don't understand what the authors want to emphasize with the sentence starting with "In spite of this," Do you mean that having small uncertainties is more important when quantifying geographical difference than for satellite validation? If my interpretation is correct, please explain why you think that is the case!

L175: I am not sure how to interpret "Correspondingly, the temporal mean of ground observation within Delta T is compared to the spatial mean of OMI data within D." As noted earlier, ground based data were aggregated into 3-minute averages. So there can only be 3 ground-based measurements in a +/- 5 min time period. Likewise, There are only one or two OMI measurements within 50 km from the ground site, based on the OMI pixel size discussed earlier. The sentence should be clarified.

L180: Specify whether the number of data pairs refers to all sites or each individual site.

L186-192: Please also provide the formulas for NSD and RMSD. In particular, I am not sure what the difference is between RMSE and RMSD. Equations would clarify this. It is also inconsistent to name the "normalized standard deviation" NSD but the "normalized

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root-mean-square difference" RMSD. It should be NRMSD. Replace "shown in x and y axis respectively" with "shown both on the x and y axis". Replace "shown as the radius from the expected point" with "shown as concentric circles around the point labeled "expected" in Fig. 3a and 3b.

L222: n was already defined as the total number of data points on line 203. Don't use the same symbol for different quantities!

L243 and Fig. 2a, b: One would expect that ground-based and OMI measurements agree better at overpass time than noon. This is indeed the case (as described) for MB and RMSE. However, surprisingly, the regression line is closer to the (ideal) 1:1 relationship for Noon_FS data compared to OP_FS data. This observation should be mentioned in the text, and reasons should be given. Perhaps this observation is a result of errors in the processing of ground-based measurements discussed earlier.

Fig. 3 and associated text. I find it very strange that overpass data have a negative bias and noon data a positive one. This points again to a problem in the calculation of one (or both) of the datasets.

Figure 5 and associated text. While I understand how the figures were calculated, I don't understand why these distributions are important to show. The figures mix in data from all sites (with greatly different cloud and aerosol conditions) and seasons. The figure depends on many variables, which I can't decouple in my head when looking at these distributions, and therefore, I don't know what to learn from these distributions. While there are differences between the distributions for the ground and OMI measurements (which ideally shouldn't exist) I wouldn't be able to grasp from this figure what might have gone wrong with the UV retrieval from OMI data (assuming that the distribution for the ground-based data is correct). I would consider removing this figure and the text that goes with it.

Fig 6 and associated text. Again, I am not sure why this figure is important. I understand that it confirms that OMI surface EDR and ground-observed EDR were drawn from the same distribution, but is this confirmation really important? What can be learned that is not already known from the correlation coefficient? Hence, I suggest to consider removing this figure also.

L286: define “peak UV”, e.g., move or copy “peak refers to the highest dose rate found in a day at each site” from the caption of Fig. 7 to the text.

Fig 7 compares two different quantities and it is therefore not surprising that there are differences. For example, the observation of clouds within the OMI pixel will always lead to a reduction of UV radiation because cloud modification factors are <= 1. In contrast, ground-based measurements can be enhanced beyond the clear-sky value during broken cloud conditions. This is a well-known phenomenon, e.g., http://doi.org/10.1038/371291a0. It is therefore not surprising that the high-frequency band is at 200 mW m-2 for OMI Noon-FS data and 220 mW m-2 for “Ground Peak” data. Since it is the goal of the paper to validate OMI data, I see little value in comparing two different quantities and concluding that “OMI solar noon time EDR may not always represent the high peak value on a daily basis due to the varying atmospheric conditions.” I suggest replacing the right panel of Fig. 7 with the distribution of Noon_FS data from the ground based measurements.

L321-325. Results discussed here and in Figure 8 do not make sense to me. Please see my General Comment earlier. For these reasons, I also disagree with the conclusion: “This indicates that the current OMI surface UV algorithm would not fully represent the real atmosphere with the assumption of constant atmospheric conditions being made and could thus induce errors in estimating surface UV irradiances.” I suspect a problem with the processing of ground-based data, not of OMI.

Having said this, I note that Bernhard et al. (2015) (see: https://www.atmos-chem-phys.net/15/7391/2015/) have reported a problem in the conversion from OMI overpass EDRs to EDRs at local solar noon by the OMI UV algorithm. They conclude “Additional analysis suggests that the pattern is likely due to a systematic error of up to +/-0.5

degree in the calculation of the local-noon SZA by the [OMI] algorithm. For a SZA of 80 degree, a 0.5 degree error in SZA results in a UVI error of about 8 %”. The authors should look at this publication and determined whether this problem also affects their data. According to my judgment, the problem is of less importance for the data of the USDA network than for data from the high-latitude sites discussed by Bernhard et al. (2015) because SZAs at the time of the satellite overpass are much smaller at lower latitudes.

L358: I believe that the larger noise in the bias at larger COT values has additional reasons than the one noted in the paper. For example, it is difficult from space to estimate the COT if the COT is large (e.g., > 10). In contrast, clouds with COT=100 attenuate UV radiation much more than those with COT=10.

Figure 13: It would be better to show trends per year or decade in percent instead of absolute values (in mW m-2 yr-1). The significant trend in one tiny part of California is almost not worth mentioning in the text (L 372).

Figure 14a: Also this plot should be presented in percent. (If you decide to keep trends in absolute terms, the unit should be changed to that of spectral irradiance: mW m-2 nm-1 yr-1.)

L377: I don’t understand why Zhang et al. (2017) found a significant positive trends over the western U.S. using OMI AOD for 2005–2015 while such trend was not found by the same author in the present work. What is the difference? The fact that this study considers measurement between 2005 and 2017 instead of 2005 and 2015?

L380: Explain acronym “AAOD” I presume it means “absorption aerosol optical depth.” Indicate that this parameter is part of the standard OMI data products. AAOD in the UV-B is very difficult to measure from space since most of the absorption is close to the ground. Indicate the uncertainty of OMI AAOD. Could the small AAOD trends (<0.003 per year) that are reported here be a result of drifts in OMI data?

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L387: Monthly averages can be greatly biased if only 10 days are available, in particular if measurements occur only at the beginning or end of a month. It would be best to repeat the trend analysis using only months with 20 or more days of data, and compare the results to ensure that trend estimates are robust and not driven by missing days.

The conclusions need to be changed to reflect changes to the text resulting from my comments above. This applies in particular to lines 437-446 where the distributions of noon/overpass data are discussed.

L454-459: The suggestion that trends in UV seen in the ground measurements are caused by trends in AAOD are highly speculative, and this should be stated. I believe that trends in AAOD derived from OMI measurements at 310 nm are rather uncertain. Furthermore, assessments of trends in AOD reported in the paper are only based on OMI (lines 377-379). The conclusions that there are no trends in AOD is premature because of the difficulty to probe the lower troposphere from space in the UV-B. It would be good to check whether the ground-based AERONET sunphotometer network has reported trends in AOD and AAOD for the period of the paper and to use those data to interpret trends in EDRs (although AERONET also does not have wavelengths in the UV-B).

Technical Corrections

General: It would be helpful to define acronyms for “overpass time EDR” and “local solar noon EDR” at the beginning and then use these acronyms in the text instead of repeating the same phrases.

P1, L13: Change “OMI data overall has ∼4% underestimate for overpass EDR while ∼8% overestimate for the solar noon time EDR” to “OMI underestimates the overpass EDR by about 4% on average and overestimates the solar noon time EDR by 8%.”

P1, L17: “with SZA” > “for SZA”

P1, L20: “viability” > “variability”

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P1, L21: Explain “Noon_FS”

L44: “in many locations” > “at many locations

L47: “In addition, the surface UV irradiance, denoted as ‘erythmal weighted”’ > “In addition, the erythemally weighted irradiance” (spectral irradiance is also surface UV irradiance in this context!)

L49: “the incoming solar radiation” > “the solar irradiance”; and “according to” > “with”

L51: “derive UV index” > “derive the UV index”

L52: “UV index” > “the UV index”

L60: “has been” > “have been” (data is plural)

L61: “the OMI data has” > “OMI data have”

L64: “in many sites” > “at many sites”

L100: “tables are the” > “tables is the”

L119: “triangular slit function with full width half maximum” > “a triangular slit function with full width at half maximum”

L177: delete “different”

L187: “normalized room-mean-square” > “normalized root-mean-square” (not “room”!)

L262 “larger” > “large”

L364: “range principally controlled” > “range that is greatly affected”

L366: “identified trend of surface EDR” > “trends in surface EDR”

L388: “In contrast, ground observation shows” > “In contrast to trends derived from OMI data, ground observations show”

L425: Explain acronyms “TEMPO and GEMS”

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