We sincerely thank the editor and reviewers for taking the time to review our manuscript and providing constructive feedback to improve our manuscript. We have revised the manuscript accordingly by following the reviewers' suggestion. Below shown are the original comments from reviewers in black and our corresponding responses in blue.

<u>Comments by RC3:</u>

The manuscript by Zhang et al., "OMI surface UV irradiance in the continental United States: quality assessment, trend analysis, and sampling issues", presents a study to evaluate the estimates of surface UV from OMI satellite measurements against ground- based measurements and additionally carries out a trend analysis. There have been several validation studies of OMI surface UV before, unfortunately it remained unclear what is the understanding that this manuscript adds to the current knowledge.

Nothing was said about the quality and uncertainties of the ground-based measurements. And since the results were peculiar enough, it was not clear whether the ground measurements tell something about OMI UV performance or whether it is other way around: comparison against OMI UV tells something about the systematic errors in the ground-based measurements. In my opinion, it takes a major revision before the manuscript can be fully evaluated. I will explain and clarify these points below.

Response: We thank you for your time reviewing the paper. As noted in the introduction (and other reviewers), prior to this study there were few studies that conducted systematic evaluation of OMI surface erythemal UV data with long-term ground-based observations in the U.S. There is also unavoidable assumption that is made in the OMI UV algorithm to derive surface UV at local noon time from satellite overpass time. This assumption needs to be evaluated and the associated uncertainties should be assessed as the local noon time is expected to have the largest surface UV radiation relevant for health studies. Furthermore, in our revised manuscript, we also emphasize the analysis of surface observation to illustrate that the peak UV at the surface is not always at local solar noon. We also assessed the OMI surface UV data in terms of their PDFs (that going beyond linear coefficient and bias). While we agree that both surface measurements and satellite measurements have uncertainties, in the revision we have emphasized the discrepancy at noon is larger than the uncertainty in surface measurements, highlighting the likelihood of sampling issues related to the inherent limitations of polar-orbing satellite such as OMI. The comparison helps us define the uncertainty enveloped in long-term estimates of the surface UV radiation from both in situ and satellite observations, assess if there are any coherent trends and differences between the two datasets, and recommend possible improvements for OMI and the need of geostationary measurements of surface UV. The last but not learn, we also show that there is no scientifically sound and coherent trends among OMI data for aerosols, clouds, and ozone that can explain the surface UV trends revealed either by OMI or ground-based estimates; nor these data can reconcile trend differences between the two estimates.

Main comments:

As the authors quite correctly mention, the information about the atmospheric conditions comes from the OMI measurement at the overpass time. So this statement is already suggesting that only the overpass time comparisons are meaningful. And indeed, my suggestion is to exclude the local noon time comparisons. However, since the authors presented also the local noon time comparisons with strange results, I want to discuss these results briefly below.

Response: We thank the reviewer for the suggestion and the comments. We understand that the comparison for overpass time would be more accurate. However, the surface UV irradiance at local solar noon time is also an important quantity as it is expected to be the largest during a day and is relevant to health studies. There is a need to quantify how much errors would be, because of the assumption of constant atmospheric conditions between satellite overpass time and the local solar noon time. Hence, we hope to keep the comparison for both satellite overpass and local solar noon time, in order to shed some light on the uncertainties from inherent limitations in the sampling made by polar-orbiting satellite. Our evaluation here not only reveals how good the OMI product is - its PDF for surface UV is in statistically significant agreement with the counterpart of surface observation, but also quantifies the uncertainties in creating surface peak UV climatology from a polar-orbiting satellite (such as OMI) that has inherent limitations in sampling. The latter is analysed here by taking advantage of long-term calibrated 3-minute surface UV data record over the U.S.

The SZA is always somewhat lower at the local noon time than at the overpass time, resulting in larger UV irradiance at the local noon by this SZA effect. This means that there has to be some very clear and systematic effect in cloudiness to compensate this, if the results in the Figure 8b are true. Cloud effect would the only plausible explanation and it would mean that at the OMI overpass time there should be systematically (on average) lower cloud amount than at the local solar noon over the stations studied. From the results of Meskhidze et al. 2009, the opposite diurnal cloud effect seems to be the case, on average thicker (higher cloud optical depth) clouds in the afternoon than before (at Terra overpass time) over most of the continental US. The overpass time of Terra is before noon, but I think this should give an indication, at least, about the sign of the systematic difference between noon and OMI overpass. So how do you explain your Figure 8b? It is absolutely necessary to include ancillary data or publication citation to convince the reader about the behavior in the Figure 8b. Otherwise, he/she is left with an impression that something has to be wrong with the ground-based measurements.

Response: We fully agree with the reviewer's point, and that is indeed one of our motivations to look into the issue: to what extent a constant atmospheric profile between the local solar noon and satellite overpass time is valid. Following your suggestion as well as the suggestion made by the second reviewer, we have double checked our analysis and found we mis-calculated the local solar noon time. Subsequently, we make corrections in the revisions. The results are interesting. Overall, OMI local solar noon time are about 95% times larger than satellite overpass time; but still, in average 22% of times, the ground measurements show that the local solar noon time surface UV is smaller than that in satellite overpass time, likely reflecting the differences in variations of clouds and other parameters (Meskhidze et al., 2009).

If the results in the Figure 8b are not understood and explained, the reader can only assume that there has to be some angular dependent error left in the measurements to cause this. And indeed, there was no single word about the uncertainty of the ground-based measurements. The reference to Lantz et al. 1999 was just given, without any further discussion. So please explain in detail the corrections applied. The correction should be ozone and SZA dependent, as they discuss in Lantz et al. 1999. From where you take the ozone values for the correction? How often the reference is calibrated? How often these instruments participate in inter-comparison campaigns? I think it would be an informative plot to show also the typical correction factor, plotted as a function of SZA, for two very different ozone amounts. The angular calibration is also a function of cloudiness, so please discuss in detail how it is included in the correction of ground-based measurements.

Response: We thank the reviewer for this specific comment related to the quality of the ground observational data used in the current work. We have revised Sect. 2.2 (ground observation data) to provide more information about the calibration and characterization process of the UMMRP broadband radiometers used in this work. The UMMRP uncertainty is ozone and SZA dependent. The erythemal UV irradiance used in the current work is prepared with the calibration factors of SZA dependence that assume the total column ozone is 300 DU. This is the best data offered by UMMRP to the scientific community in the public domain. We understand that the variation of O_3 amount may lead to errors in the calibration, and in the long-term, there are conditions where O_3 amount can be larger than 300 DU, and there are also times where O_3 amount is less than 300 DU. Kimlin et al. (2005) has shown overall UMMRP broadband radiometers have an uncertainty on the average of ± 6%.

Currently the trend analysis part does not offer anything consistent. For the careful reader, the main message seems to be that the ground-based measurements result in both negative and positive trends, no matter how the data are selected, and even for stations that are almost side by side. So perhaps this trend analysis could be also excluded, or the authors explain what is the consistent message it brings. For instance, let's take a look at the East coast of US, some sites give slightly negative trend (13b), the southern most shows a positive trend. If one studies Zhang et al. 2017 in detail, it is clear that there is no AAOD trend in this region, while there is a negative trend in AOD (from OMI, but also from other instruments). So one would assume slight positive trend in surface UV, but in the contrary there is negative trend in two sites.

Response: The trends derived from ground measurements are within the measurement uncertainty range. Our original hypothesis is to see negative trend because of recovery of O_3, but on the other hand, aerosols amounts are also declining. Our analysis eventually proves the null hypothesis there is no coherent trend. Indeed, we found there is no scientifically sound and coherent trends among OMI data for aerosols, clouds, and ozone that can explain the surface UV trends revealed either by OMI or ground-based estimates; nor these data can reconcile trend differences between the two estimates. While it would be nice and interesting to see a coherent trend, proving a null-hypothesis is equally scientifically important.

The changes in AAOD alone cannot explain these trends, so it is absolutely important to consider the simultaneous changes in AOD as well. If one selects those regions from Zhang et al. 2017 where both AOD and AAOD (from OMI) shows positive change, then the most probable sites are Holtville, CA and Las Cruces, NM, where based on this change, one would assume to see negative change in surface UV. However, both stations show positive change. These are just two examples why I argue that in its current form, the manuscript fails to offer consistent and convincing message about the trend analysis. My two main comments might be linked: if the quality of the ground-based measurements is not sufficient and/or properly considered, then these issues might become visible both in the results shown in Figure 8b and also in the trend analysis. It can be also, that in any case, the signal is too weak to detect any meaningful trend, but if so, then the discussion and comparison against OMI AAOD is not justified.

Response: We agree that the key message should be clearly delivered. Overall findings are as followings:

1) at satellite overpass time, 7 % overestimation by OMI in comparison with surface measurement, and this bias, while close to, is definitely larger than 6 % uncertainty range of surface measurements.

2) at local noon time, in average, OMI noon time EDR is 18 % larger than at satellite overpass time, but 22 % of times, noontime is smaller derived from the ground measurements.

3) at local noon time, the satellite-based OMI is 7% bias higher than surface counterpart, and this bias is out of the surface measurement range.

4) we find several sides shows statistically significant trend, but the trend magnitude overall is within the surface measurement range. There is no scientifically sound and coherent trends among OMI data for aerosols, clouds, and ozone that can explain the surface UV trends revealed either by OMI or ground-based estimates; nor these data can reconcile trend differences between the two estimates. While it would be nice and interesting to see a coherent trend, proving a null-hypothesis is equally scientifically important

Specific comments:

Line 364, you mention that the absorbing aerosols could be the reason for the OMI UV trend. It is not the likely reason, since the correction is taken from monthly climatology. So what did you mean?

Response: We agree with the reviewer that the absorbing aerosols will not be likely the reason for the OMI UV trend detected. Other factors such as the scattering aerosols concentrations, clouds and trace gas concentrations could all contribute to the trend. The key message here is that on the first order, there is no apparent trend in the factors that affect the surface UV, and there is also no apparent and spatially coherent trend in OMI UV data.

Line 370, there is a better reference to Kinne et al. (Kinne et al. 2013 below).

Response: Thanks for the suggestion and we have updated the reference.

Line 361, if you include 310nm, perhaps comparison to the 380nm trend would bring something useful, since it does not have any significant ozone absorption.

Response: Thanks for the good suggestion and we have also studied the trend of OMI spectral irradiance at 380nm (please see Fig 8(e)).

Table 3. If you used ordinary least squares for regression, please remember that it gives a systematically biased slope when there is uncertainty in x-axis. These numbers, slopes in particular, would be informative if the method for the regression is correct one. See Cantrell et al. 2009 or Pitkanen et al. 2016. Please explain the method that was used and the possible limitations.

Response: Thanks for bring us the attention of those two references. We have explained the regression method used in the current work and future work should be paying more attention to the linear regression methods used as discussed in these two reference papers.

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