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Atmospheric Chemistry and Physics Discussions

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 #3

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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.

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.

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.

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.

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.

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.

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?

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

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.

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.

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