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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Global Distribution and 14-Year Changes in Erythemal Irradiance, UV Atmospheric Transmission, and Total Column Ozone 2005–2018 Estimated from OMI and EPIC Observations" *by* Jay Herman et al.

Anonymous Referee #2

Received and published: 9 November 2019

**** Overview

The manuscript by Herman et al. discusses trends in total ozone, atmospheric transmission and erythemal UV irradiance for 2005 – 2018 that were calculated from measurements of the Ozone Monitoring Instrument (OMI) on NASA's polar-orbiting AURA satellite. Furthermore, images of the distribution of the UV Index and scene reflectivity across the sunlit Earth are presented, which were measured by the Earth Polychromatic Imaging Camera (EPIC). This instruments is onboard the Deep Space Climate Observatory (DSCOVR), which observes the Earth from the Lagrange L1 point, located

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Discussion paper

between the Sun and the Earth. The paper presents an update on UV trends measured by OMI and provides stunning images from a new vantage point. Both aspects are of interest to readers of ACP. Unfortunately, the manuscript has many shortcomings (see below) and I feel that the paper requires major revisions before publication could be considered.

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**** General Comments

1- Problems in statistical analysis

The authors calculate trends in erythemal irradiance via least squares regression analysis from data that are not deseasonalized and greatly autocorrelated (e.g., Figures 1A and 1B). While they do not specify their linear regression method, I assume that it is the "ordinary least squares" (OLS) method. This method requires that data are not autocorrelated (e.g., there should not be a serial correlation between the residuals of the regression). If the OLS method is applied to autocorrelated data, the OLS estimator (e.g., trends in erythemal irradiance) may not be the best estimator and the errors of the trends tend to be too small, resulting in "significant" trends when in fact they are not significant.

The author describe on L111 that they have compared their trend estimates with results from the "standard multivariate method" by Guttman (1982). They conclude that the results of both methods "approximately agree" and refer to Table A5. While I do not have access to the work by Guttman and can't comment whether this method is appropriate for processing the author's erythemal time series data, I disagree that the results of the to methods agree "approximately". Specifically, trends disagree by up to 33% (i.e., the trend for Buenos Aires calculated with the least squares "UTD" method is -0.20% per year while that of the method by Guttman is -0.15% per year). Differences for the uncertainty (standard deviation) of the trend estimates are even larger, reaching 50% for Buenos Aires (0.21 versus 0.14).

To better understand the magnitude of errors arising from applying the OLS method to greatly autocorrelated data, I have set up a hypothetical time series consisting of a scaled and shifted sine function. Parameters were chosen such that the function varied between 1 at the winter solstice and 10 at the summer solstice. The function was repeated 14 times to roughly simulate the UV Index at a mid-latitude location between 1 January 2005 and 1 January 2019, with one data point provided every day. In a second step, I multiplied this function with a linear trend function of the form "T = 1+Day*0.01/365 - Offset" to simulate a linear trend of 1% per year. ("Day" is a continuous day counter and "Offset" was chosen such that T = 0.93 on 1 January 2005 and T=1.07 on 1 January 2019.) I then calculated the trend with the OLS method, resulting in a trend per year of 0.845% +/- 0.393% (95% confidence interval). So the calculation resulted in a trend that is about 16% lower than the actual trend of 1% per year. Lastly, I calculated annual averages from the time series and applied the OLS method to these averages, resulting in the correct trend of 1% per year. These results suggest that the trend estimates provided in the manuscript for mid-latitude sites are about 16% too low.

Since trend estimates are at the core of the manuscript, the authors should reconsider their method to calculate trends, and re-calculate their trends if their assessment confirms my suspicion that the original trend estimates are erroneous. I also suggest to calculate annual averages and apply a OLS analysis to those, as I did in my hypothetical scenario. This method should not be subject to biases because the underlying UTD data are equally spaced and without data gaps. Annual averages should be unaffected by autocorrelation, although effects like the solar cycle may lead to weak autocorrelation.

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2- Potential drift in OMI data

I am surprised that significant ozone trends were found for many ground stations (Table

A4) and even zonal means (Figures 16C, Figure 17 bottom, 19A), considering that the last WMO Scientific Assessment of Ozone Depletion: 2018 report concludes on page ES.26 that “No statistically significant trend has been detected in global (60°S–60°N) total column ozone over the 1997–2016 period (Figure ES-1)” For example, Figure 16C strongly suggests that ozone is recovering for latitudes between 55° S and 20° N, while such a recovery could not be demonstrated in the WMO report. The reasons for this inconsistency should be discussed because the question of whether or not the ozone layer is healing in response to actions prompted by the Montreal Protocol is of great interest.

According to L531 of the manuscript, “The NASA OMI project suggests that there may be an OMI drift of +0.1% per year (private communication) relative to a reference TCO3 data set derived from the overlap (2012 – 2018) with NOAA 19 SBUV/2 (National Oceanographic Atmospheric Administration Solar Backscatter Ultra Violet-2) instrument. The effect of this systematic drift would be to shift the curve in Fig. 19A downward by 0.1%/Year or be considered as an uncertainty that is greater than the small statistical uncertainties.” This revelation should be greatly expanded, for example, by comparing OMI measurements not only with NOAA 19 SBUV/2 data but also ground-based reference measurements by Dobson and Brewer instruments. Such a cross-check against ground-based measurements is standard practice (e.g., Bodeker et al. (2001)).

While it has been stated in the manuscript that observations of the Antarctic high plateau were used to correct drifts (L538), it is not clear whether this method also works for channels affected by ozone. Of course, if the ozone trends were to be affected by artifacts, erythemal trends (e.g., Figure 14A) would also be compromised.

The discussion on potential drift in OMI data should be moved to Section 2 where OMI data are introduced. If OMI drifts indeed by +0.1% per year, the surprisingly large and statistically significant zonal trends, both in ozone and in erythemal radiation, that are shown in many figures could disappear. This would be consistent with the WMO report mentioned earlier.

It would be unfortunate if the results of this paper would be used to “prove” that the ozone layer is now recovering over the latitude range of -30° to +20° and it is later discovered that the apparent recovery was due to a drift of OMI. The question of whether or not OMI ozone data are drifting, and to what degree, should be a central point of the paper and not hidden as a remark with a “private communication” reference.

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3- Simplifications in calculating erythemal data at the surface from OMI raw data

Erythemal UV data are being operationally calculated from OMI data in near real time by the OMI team and are provided both in gridded form (e.g., https://disc.gsfc.nasa.gov/datasets/OMUVBd_003/summary) and for many locations (https://avdc.gsfc.nasa.gov/index.php?site=2057856112&id=79). Processing schemes are more sophisticated than the application of the formulas used by the authors (summarized in the manuscript’s appendix). For example, the effect of absorbing aerosols is considered by the OMI team (at least as good a it is possible from OMI observations) while according to L611 “absorbing aerosols are not included” in the manuscript. The authors should acknowledge and reference the work of the OMI team and the UV data products provided, and should compare their erythemal data with these data, at least for several sites. They should also explain why their trend analysis is based on data derived from parameterizations instead of data processed by the OMI team.

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4- Bias of OMI erythemal data

In general, the paper overstates the ability to accurately determine the UV irradiance at the surface with measurements from space, in particular in the presence of (absorbing) aerosols and clouds. As also stated by Referee #1, OMI UV data have been compared with ground-based measurements at many sites. According to most publications, OMI UVI data agree within a few percent (e.g., within the combined uncertainty of OMI and ground-based measurements) under clear-skies at unpolluted sites. However, OMI data greatly overestimates the UV Index at the surface at heavily populated regions in the presence of absorbing aerosols. For example, at Santiago, Chile, OMI overestimates the UVI by about 47% according to Cabrera et al. (2012). OMI also tends to overestimate the UVI at the surface under cloudy conditions. For example, Fan et al. (2015) found that OMI overestimates the UVI at a New Jersey site by 24% on average for overcast conditions while the bias under clear skies is less than +/-2%. Since the bias depends on cloud optical depth, any trend estimates in the UVI caused by long-term changes in cloud cover will also be subject to error. Trend estimates provided in the manuscript for locations that are either affected by absorbing aerosols or by changing cloud cover (quantified by the LER) may therefore differ substantially from the actual trend at the surface. The authors should acknowledge this and discuss that trend estimates from satellite data have their limitations because of the satellite's limited ability to probe the lowest layer of the atmosphere, in particular under cloud cover.

I also like to note that the effect of absorbing aerosols on the UVI is very difficult to determine from space because the wavelength-dependence of the single scattering albedo (SSA) in the UV-B is largely unknown. Recent research (Mok et al., 2018) has shown that SSA measurements performed in the UV-A and visible range (e.g., by AERONET) cannot be simply extrapolated to 310 – 315 nm, which is the wavelength range where the erythemally weighted solar spectrum peaks. Likewise, the decrease of erythemal UV radiation at the surface caused by clouds depends on many factors such as cloud type, cloud fraction, presence of aerosols, viewing geometry, etc., and cannot be determined perfectly from LER.

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5- Missing link between OMI and EPIC measurements

The manuscripts describes measurements by OMI and EPIC but provides almost no links between the two datasets. I suggest that the authors add a section where they compare measurements of the two instruments. For example, for most images taken by EPIC, there should be a measurement of the UVI by OMI, taken at the same time. So matchups for specific EPIC pixels should be possible. Such comparisons could help to discover potential systematic errors in the data of the two systems.

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6- Inconsistent notation

It is confusing that different symbols for the same quantity are used in the text and appendix of the paper. For example, zeta was defined as the latitude in line 12 and as the SZA in line 157. Considering that the symbols used in the Appendix are identical to those of previous publications by the authors (e.g., Herman et al. (2010) and Herman (2018)), I suggest that these symbols are also used in the main text. This would mean that theta should be used for the SZA throughout the paper, including Eq. (2), where for inexplicable reasons xi was used for the SZA. I also note that the symbol T is used for the "fractional cloud + haze transmission" in the text but C_T is used in the Appendix. The authors should ensure that symbols are used consistently throughout the manuscript.

7- Excessive length of manuscript

I agree with Referee #1 that the manuscript is very long and somewhat repetitive. However, the length of the paper doesn't affect its readability because a substantial portion consists of EPIC images and their descriptions, which can be easily navigated. I leave it up to the authors and the editor to decide whether a reduction in length is necessary.

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**** Specific Comments

The abstract should include a sentence describing how results from OMI and EPIC compare.

L21: Change “high northern latitudes” to “northern mid-latitudes” (“High latitudes” typically refers to 60° to 90°, not 40° to 60°.

L41: These references are more than 20 years old. Please also include more recent works!

L50: “skin. erythemal” > “skin. Erythemal”

L71: Please provide a reference to back up the assertion that OMI is “well calibrated”.

L108: “data gaps“ should also be mentioned here.

L126: Change “UVI = E/25 mW/m2” to “UVI = E/(25 mW/m2)” (Since E is expressed in units of mW/m2, dividing by 25 mW/m2 will result in a dimensionless UV Index of the correct magnitude.)

L127: Change “14-year annual cycles (1 January 2014 to 31 December 2018).” to “14-year annual cycles (1 January 2005 to 31 December 2018).” (2014 is incorrect).

L128: There is no Fig. 1, only Fig. 1A and 1B, which have separate captions.

L167: In the standard definition, the Radiation Amplification Factor (RAF) depends on both SZA and ozone. See for example, page 20 - 21, and Figure 6 of Seckmeyer et al. (2006). In particular at large SZAs, the RAF depends greatly on ozone. I am therefore surprised that neither the exponent of Eq. (1) nor the term U(zeta) depends on ozone. This should be explained and uncertainties in trend calculations arising from the omission of the ozone-dependence should be quantified.

As a side note, I like to mention that a formula similar to Eq. (1) for estimating the clear-sky erythemal irradiance from SZA and ozone has also been suggested by Madronich (2007). The formula also uses an ozone-independent RAF. Figure 1 of this paper shows errors in the order of 10% arising from the omission of the RAF’s ozone dependence.

L169: Please explain the term U(zeta) in Eq. (1).

L170: Here the SZA is denoted with theta while zeta is used on the remainder of the page. However, as noted in my General Comments, I urge the authors to use consistent symbols throughout the manuscript, and I suggested to use theta for the SZA because this is the symbol used in previous publications of the author.

Eq. 2: This equation uses xi for the SZA. Please use consistent symbols!

Caption Fig. 4: Please specify the filter that was used to smooth the measured data (indicated as symbols). It also seems that there are fewer than 365 data points in the year 2005. This is likely because OMI does not provide a overpass everyday close to the equator. If true, this should be mentioned.

L229 - 236: Somewhere here it should be mentioned that neither OMI or EPIC data will report erythemal irradiance above the clear-sky limit. It has been shown in numerous measurements from ground-based instruments that erythemal irradiance can occasionally exceed the clear sky limit during broken clouds when the direct solar beam is not attenuated by clouds and the diffuse fraction is increased due to reflections from clouds in the vicinity of the Sun.

L255: I do not understand "All the sites have a clear annual cycle compared with the Northern Hemisphere sites." Also the NH sites have a clear annual cycle, with UV low in winter and high in summer.

L262: Why should the minimum SZA at Ushuaia occur in January? It occurs on the day of the summer solstice, around 21 December.

Figure 5 and caption Figure 5: Total column ozone was previously abbreviated with TCO3. Here it is TC(O3). Please strive for consistent acronyms throughout the manuscript!

L276-L279 and L323-L327: In addition to the measurements reported by Cede et al (2002, 2004), the UVI at Ushuaia was measured for 20 years (between 1988 and 2008) with a spectroradiometer of NSF UV Monitoring program. A climatology of these measurements is available in Bernhard et al. (2010). According to these results, the maximum UV Index measured in October was 11.5 when the ozone hole was moving over the sites. This value is only slightly smaller than the overall maximum of 11.6, measured on 26 November 1996. So at least for this historical period, the highest values were not measured in December, when the SZA is smallest, but in October and November, when ozone was exceptionally low. In addition, the average UV Index at Ushuaia peaks in early January (not February as suggested on line 324) and is about 5.5. Of course, these results refer to a different period than the OMI period, however, the fact that there have been ground based measurements at Ushuaia for 20 years should at least be mentioned.

L358: The sentence "In these images, local solar noon is near the center, but offset by EPIC's viewing angle that is 4° to 15° away from the Earth-sun line." should be better explained. (I presume that the obvious shift of the highest UVI values relative to the center of the images is due the effect that EPIC is not located exactly at the L1 point but is instead orbiting this point.)

Figure 10B: I suggest to indicate the location of Mt. Everest in the figure.

The figures shown on page 21 and 22 are both labeled "Figure 11".

L398: Has there been any validation of the parameterization describing the increase of the UVI with altitude? I would expect that the altitude effect is non-linear (e.g., the higher one goes, the less atmosphere is overhead and the less UV radiation is Rayleigh-scattered downwards), and parameterizations of the effect (e.g., Eq. (A10)) that were established using data from lower elevations may not be appropriate for the altitude of Mt. Everest.

Caption Figure 13. The lowest two panels should be labeled "E" and "F" and the caption should be change from "...by the dark horizontal bars..." to "...by the dark horizontal bars in Panels E and F..."

The labeling of the figures on pages 24 and 25 is confusing. Figure 13 on page 24 has 6 panels, four of which have labels A, B, C, or D (and I suggest to add labels E and F). The figure on page 25 should either become Figure 14, or the two panels should be labeled 13G and 13H. It would be best if all eight panels were shown on one page in the printed publication.

L457: 3.5 Zonal averages and 14-year trends are two different things. I suggest to break this subsection in two and use different headings.

L463: I don't understand "This includes longitudes containing high altitude sites at moderately low latitudes where the local UVI maximum can reach 18 to 20." If the Andes were in the center of the image, the UVI at latitude 0 would be in the 18 to 20 range. So the zonal maximum for latitude 0 would also be that high. Please explain why this is apparently not the case.

With respect to Figure 14B, I am puzzled that the station-to-station variability seems to be considerably larger than the variability that I would expect from the relatively small error bars (realizing that these refer to 1 sigma, not 2 sigma). If these data had been collected by ground-based stations, I would expect such variability because every instrument at the ground can drift with a different rate. However, this is not the case for OMI, so I would expect a better consistency between station-to-station variability and the errors bars for individual station. Can this inconsistency be explained?

Figure 17: Trends in erythemal irradiance and transmission shown in Figure 17 feature variations on a $\sim 5^{\circ}$ latitude scale. Are these fluctuations systematic or random? For example, if the figures had been drawn at longitudes of 25° E and 160° W, would the patterns be radically different? Also note that data points are plotted every 5° in latitude, not 10°, as the caption indicates.

L546: “dangerously” is subjective. The official word for UVI >=11 is “extreme”.

L549: The conclusion that “nearly half the sites have shown 2-sigma changes in UVI” may need modification if the drifts of OMI mentioned earlier turn out to be true or if the simplifications of the regression analysis also discussed earlier resulted in spurious significant trends.

L555: The conclusion that ozone has increased between 55° S to 35° N may also need modification for similar reasons as in the previous point.

L585: Erythemal irradiance is calculated from the solar *spectral* irradiance in W/m2/nm, not the irradiance in W/m2. It should also be mentioned that the solar spectral irradiance is the sum of the spectral irradiance from the solar beam and diffuse spectral irradiance from the sky on a horizontal plane at the surface.

L594: As mentioned earlier, I don’t understand how R can be independent of ozone. I believe R(theta) is an approximation which works within acceptable bounds, but would likely fail under the ozone hole when the solar zenith angle is very large and total ozone is 100 DU.

Eq. (A4): Change “exˆ4” to “e thetaˆ4”

Eq. (A6): Please mention in the text that H scales the erythemal irradiance at the surface to an altitude z. To explain the calculation of H, it would be better to say that H was calculated by fitting a function to the ratio of RE = E/E0 where E and E0 were calculated with TUV.

Tables A1 and A2: Do the coefficients specified in the two tables really have to be provided as double-precision numbers? I am aware that these coefficients have already been used in previous works by the authors, but I am puzzled that parameterizations were chosen that require coefficients at such high precision. It would be good to add a sentence why such high precision is required.

L614: Why is trend significance based on a confidence level of 96%? 95% is the norm for studies like this (Although “2-sigma” technically corresponds to a confidence level of 95.45%, which 95% when rounded).

Caption Figure A1: R(theta) and U(theta) are functions, not coefficients.

L625-627: The paper only discusses results for erythemal irradiance. This paragraph can be deleted as the action spectra discussed here have no relevance to the paper.

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**** Technical Corrections:

“Sun” is spelled lower and upper case. Please use upper case spelling (consistent with upper case spelling of Earth) throughout.

L28: There should be two closing brackets after “(3.78 km)” to match the opening bracket start at “(e.g.,”

L69: “latitude dependent” > “latitude-dependent”

L86: “occasionally a 2nd” > “occasionally 2nd”

L98: “and discussed in” > “which are discussed in” (otherwise “discussed in” refers to “The numerical algorithm” which is discussed in the Appendix and not in “in separate sections of this paper.”)

L260: “a lowest” > “the lowest”

L313: Fig. 6, > Fig. 6B,

L315: The part of the sentence starting with "resulting in a difference in . . .” sound very awkward. Please improve!

L404: “ae quite” > “are quite”

L486: “Atmospheric Transmission,” should be lower case

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