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 #1

The article has been rewritten and reorganized, The OMI and EPIC data are in separate sections Aerosol absorption has been added. Multivariate linear regression has also been added. Table A4 has been expanded. The OMI LER calibration correction is now shown in the appendix

**** General Comments Note Yellow are replies to the reviewer and grey is for text included in the manuscript

This paper focuses on the analysis of the global distribution and changes (2005-2018) of UV erythemal irradiance (also UV Index) retrieved from OMI and EPIC satellite data. Overall, it is a good and useful paper. Nevertheless, the next two specific points must be carefully revised before publication:

Specific comments:

1. The manuscript must be reduced because in my opinion it is too long.

The manuscript is still long. Combining the results from 2 satellites makes a long paper necessary

This reviewer proposes some suggestions but the authors should perform a great synthesis exercise in order to lead the manuscript to a smaller size (and more readable) than the current one:

- In Section 2 “Erythemal Time Series and LS Linear Trends” which reports results from OMI, Figures 2A, 2B, 6A and 6B can be removed together with their discussions because of they are related to EPIC. Additionally, the comparison between Northern and Southern sites (Lines 322-331 and Figure 7) is within Subsection 2.3 “Southern Hemisphere”. Please removed it or add to a new subsection.

EPICresults have been moved to their own subsection

- In Section 3 “Global view of E distribution from EPIC”, the analysis of Everest data could be removed (Lines 393-405 and Figures 10A-10B).

The Everest figure has been removed

In addition, the subsection 3.5 “Zonal average E and 14-years trends” shows results from OMI data instead of EPIC (see captions Figures 16, 17, 18 and 19).

Zonal averages have been calculated for EPIC but not OMI. For OMI the latitude dependence is derived for 191 cities and 60 ocean sites, the latter with no snow

2. The results reported in this manuscript derived all from satellite instruments, mainly the UV radiation trend, must be compared with results derived from ground-based stations. This reviewer misses this type of comparison in the discussion of the results which could clarify the quality of the satellite data. The authors should add to the discussion more references about papers with analysis of UV trends using well-calibrated and well-maintained instrumentation at surface. Here some possible works to cite in the manuscript:

A comparison with ground-based stations is not included for the magnitude of the erythemal irradiance, but not long-term trends. I added more references where appropriate and have included ground-based comparisons (see Table 2) to verify the amount of erythemal irradiance. Long-term noontime data from most Brewer sites is not readily available.

Bernhard, G., C. Booth and J. Ehramjian, Climatology of ultraviolet radiation at high latitudes derived from measurements of the National Science Foundation's Ultraviolet SpectralIrradiance Monitoring Network,in UV Radiation in Global Climate Change,ed. W. Gao, J. Slusser and D. Schmoldt, Springer, Berlin Heidelberg, 2010, pp. 48–72.

Eleftheratos, K., S. Kazadzis, C. S. Zerefos, K. Tourpali, C. Meleti, D. Balis, I.Zyrichidou,K.Lakkala,U.Feister,T.Koskela,A.HeikkilaandJ.M.Karhu,Ozoneandspectroradiometric UV changes in the past 20 years over high latitudes, Atmos.-Ocean, 2015, 53, 117–125.

Fountoulakis, I., A.F.Bais, K.Fragkos, C.Meleti, K.TourpaliandM.M.Zempila, Shortandlong-termvariabilityofspectralsolarUVirradianceatThessaloniki,Greece: effects of changes in aerosols, total ozone and clouds, Atmos. Chem. Phys., 2016, 16, 2493– 2505.

Hooke, R.J., M. P. Higlett, N. Hunter and J. B. O'Hagan, Long term variations in erythema effective solar UV at Chilton, UK, from 1991 to 2015, Photochem. Photobiol. Sci., 2017, 16, 1596–1603.

Krzy´scin, J.W., and P. S. Sobolewski, Trends in erythemal doses at the Polish Polar Station, Hornsund, Svalbard based on the homogenized measurements (1996–2016) and reconstructed data (1983–1995), Atmos. Chem. Phys., 2018, 18, 1–11.

Liu, H., B. Hu, L. Zhang, X. J. Zhao, K. Z. Shang, Y. S. Wang and J. Wang, Ultraviolet radiation over China: Spatial distribution and trends, Renewable Sustainable Energy Rev., 2017, 76, 1371–1383.

Román, R., J. Bilbao and A. de Miguel, Erythemal ultraviolet irradiation trends in the Iberian Peninsula from 1950 to 2011, Atmos. Chem. Phys., 2015, 15, 375–391.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-793, 2019.

Anonymous Referee #2

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 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.

The article has been rewritten and reorganized, The OMI and EPIC data are in separate sections
Aerosol absorption has been added. Multivariate linear regression has also been added.
Table A4 has been expanded. The OMI LER calibration correction is now shown in the appendix

**** General Comments Note Yellow are replies to the reviewer and grey is for text included in the manuscript

Problems in statistical analysis. I am now using a standard analysis that does give different answers. The method is described in the revised version along with references.

2.1 Multivariate Linear Regression Model for Calculating LS Trends.

Trends B(t) were determined for Erythemal time series E(t) (similar for total column ozone and cloud transmission time series) using a generalized multivariate linear regression (MLR) model (e.g., Randel and Cobb, 1994, and references therein):

$$E(t) = A(t) + B(t)\cdot t + R(t) \tag{4}$$

where t is the daily index (t=1 to 5113 for 2005–2018), A(t) is the seasonal cycle coefficient fit, B(t) is the linear LS trend coefficient fit, and R(t) is the residual error time series for the regression model. A(t) involves 7 fixed constants while B(t) is a single constant. The harmonic expansion for A(t) is

$$A(t) = a(0) + \sum_{p=1}^{3}\left[a(p)\cos(2\pi pt/365) + b(p)\sin(2\pi pt/365)\right] \tag{5}$$

where a(p) and b(p) are constants. Statistical uncertainties for A(t) and B(t) were derived from the calculated statistical covariance matrix involving the variances and cross-covariances of the constants (e.g., Guttman et al., 1982; Randel and Cobb, 1994). The linear deseasonalized trend results for various sites are listed in Tables 1, 3,4, and A4 in percent per year with one standard deviation (1σ) uncertainty. For comparison of the trends and trend uncertainties derived from (5), trend analysis was also done using monthly average data (one data point per month). The trends and 1σ trend uncertainties derived from the monthly averages were found to be nearly identical to trends and 1σ uncertainties derived from the daily data.

References:

Guttman, I., *Linear Models: An Introduction*, Wiley Interscience, 358 pp, 1982.

Randel, W. J., and J. B. Cobb, Coherent variations of monthly mean total ozone and lower stratospheric temperature, 99, D3, 5433-5447, https://doi.org/10.1029/93JD03454, 1994.

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 "signi?cant" trends when in fact they are not signi?cant.

The calculation of trends is now by a standard Multivariate Linear Regression Model which does give different trends.

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 two methods agree "approximately". Speci?cally, 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).

Not applicable in the revised text

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% con?dence 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 con?rms 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.

The following section has been added

1.1 UV absorbing Aerosols

The algorithm used to estimate E in this study is a fast polynomial fit algorithm FP (Herman, 2010; 2018) based on calculations using the scalar TUV radiative transfer program (Madronich , 1993a; 1993b; Madronich and Flocke, 1997). The FP algorithm used to estimate E has been enhanced to include the effect of aerosol absorption on UV irradiance based on derived aerosol optical depths from OMI measured radiances (Torres et al., 2007). The measured absorbing OMI aerosol optical depths τ_A (354nm) corresponding to the locations in Table A4 are available from https://avdc.gsfc.nasa.gov/pub/DSCOVR/TimeSeries_Aerosols/.

The wavelength λ dependence of τ_A is approximately given by using the absorption Angstrom exponent 1.8 derived from data obtained over Seoul, Korea in a manner similar to that derived for Santa Cruz, Bolivia (Mok et al., 2016).

$$\frac{\tau_A(\lambda)}{\tau_A(354nm)} = \left[\frac{\lambda}{354}\right]^{-1.8} \qquad (1)$$

The reduction factor C_A for irradiance E caused by absorbing aerosols is given by Eqn. 2.

$$C_A = E(\tau_A(\lambda))/E(0) = (1 + 3\tau_A(\lambda)) \qquad (2)$$

For the purposes of estimating the absorption optical depth for erythemal irradiance a single wavelength, λ = 310 nm, is used approximately corresponding to the maximum of the product of solar flux in the troposphere and the erythemal action spectrum, from Eqn. 1

$$\tau_A(310\ nm) = 1.27\ \tau_A(354\ nm)$$

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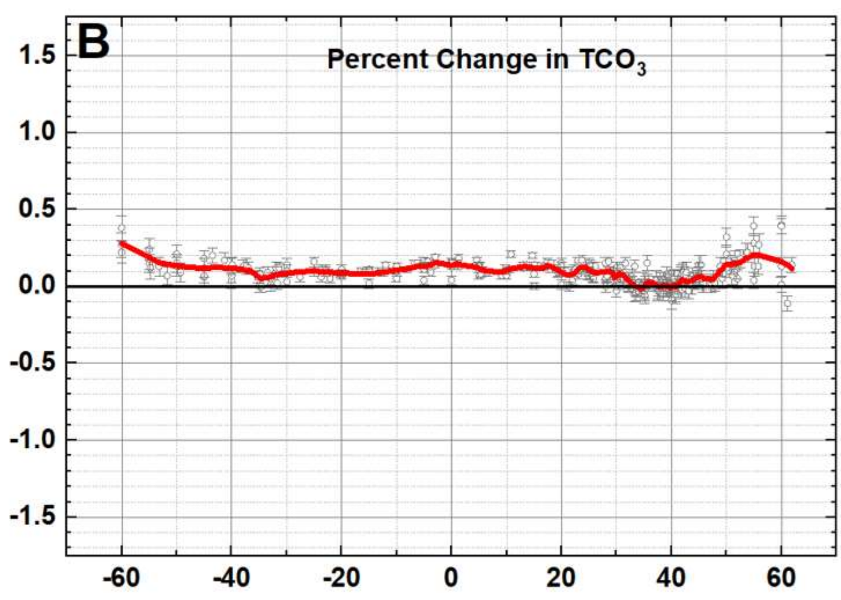
2- Potential drift in OMI data

I am surprised that signi?cant 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.

The ozone data set is the standard version that is not corrected for LER drift. Only the LER data were corrected for drift

Ozone trends are not significant at most stations at the 2 standard deviation level. A few stations are showing significant changes, mostly at higher latitudes. However, a Loess fit, the least squares equivalent of a running average over 15^{O} of latitude suggests, that ozone has increased slightly.



Ozone trends with 1σ error bars from Fig. 7A

The drift has only been applied to the LER, which ground-based instruments cannot measure.

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)).

LER OMI Drift The details will be the subject of a future paper

A graph showing the LER correction has been added in the appendix with a brief explanation. The ozone values are not significantly changed so the original ozone values from OMI are used.

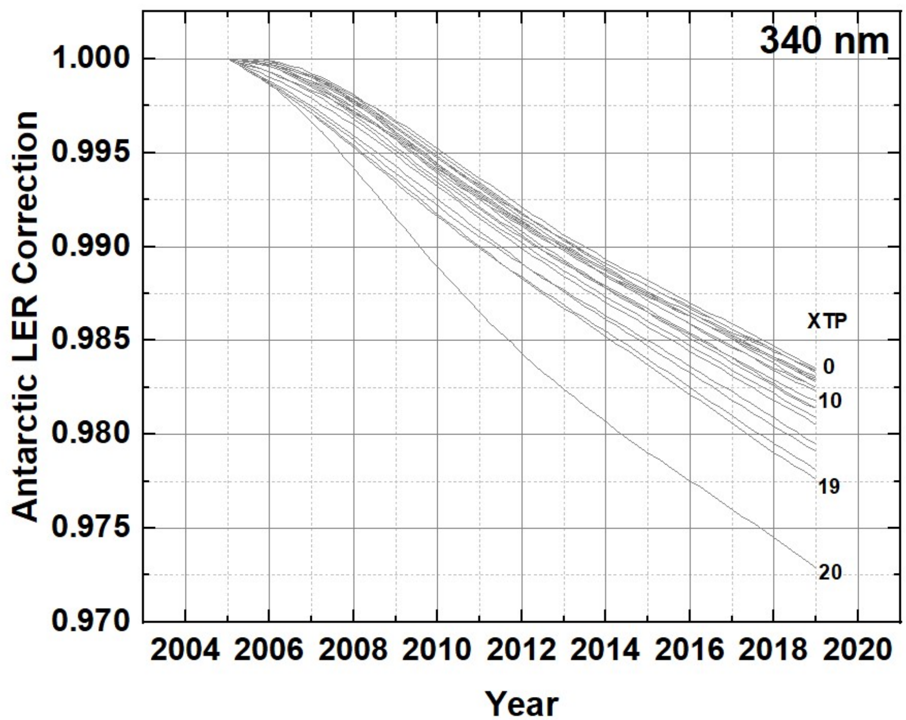


Fig. A2 Correction factors for change in OMI sensitivity at 340 nm by measuring ice reflectivity over the Antarctic high plateau. For cross track positions XTP 0 to 19, the change has been less than 2.5%.

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 original OMI ozone data are used, since the 2.5% 14 year drift will have only a small effect on ozone amounts and trends unless it can be shown that there is a strong wavelength dependence in the drift. So far, this has not been found.

The correction was not applied to the channels affected by ozone, only to the LER. Standard ozone values from OMI were used to estimate the Erythemal irradiance. The OMI-TOMS type algorithm uses ratios of specific channels plus the 340 nm channel LER. The ratios of the short wavelength channels would not be significantly affected by the small correction. The contribution of an error in the 340 nm reflectivity is small. For clear-sky scenes a 1% error on LER would be less than 1 DU. For cloudy scenes, a bigger error is estimating the amount of ozone under the cloud.

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.

The trend analysis has been revised as you recommended. A plot of all the sites is now

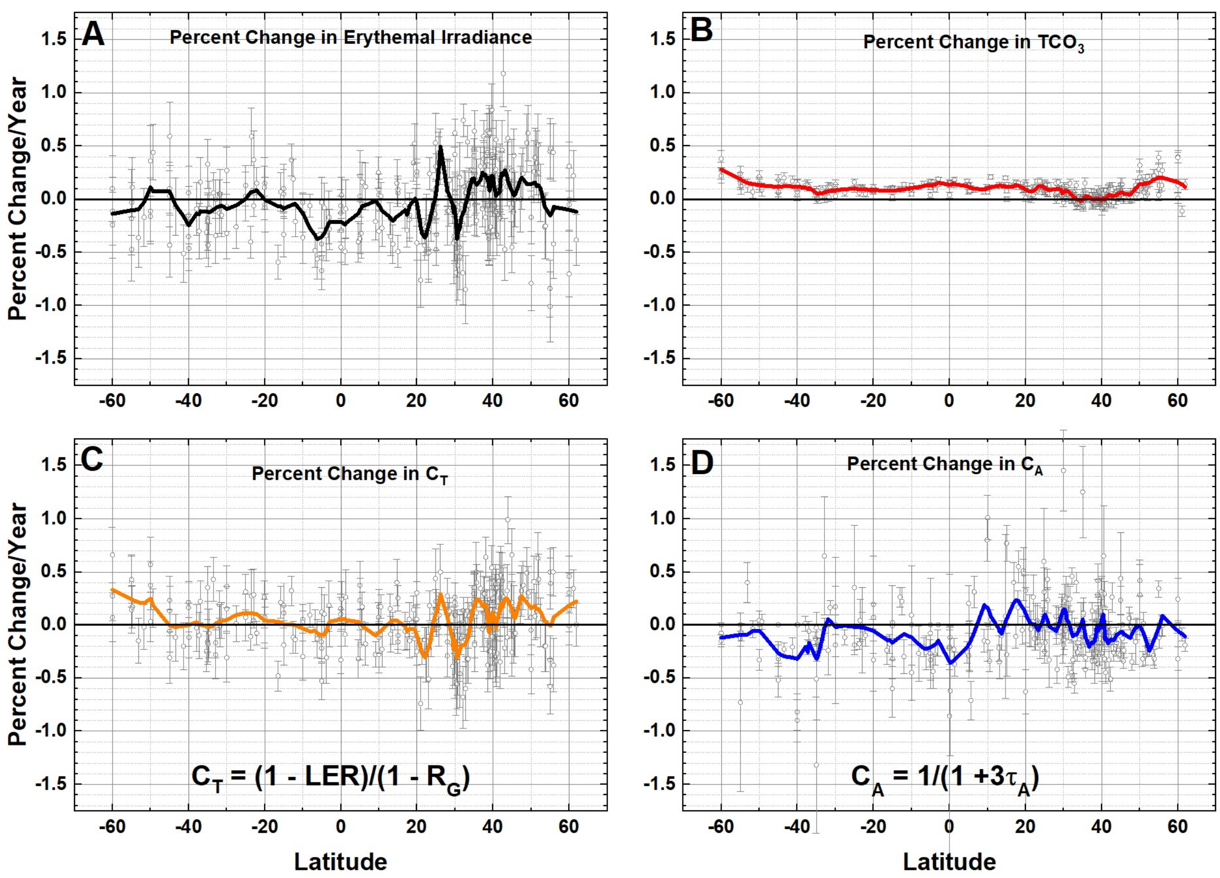


Fig. 7A Percent change per year for (A) Erythemal Irradiance, (B) Column Ozone for the period 2005 – 2018, (C) Atmospheric Transmission, and (D) Absorbing Aerosol Transmission from OMI observations at individual sites (see Table A4). The solar cycle and quasi-biennial oscillation effects have not been removed. Error bars are 1σ. Solid curves are Loess(0.1) fits to the data (15^{O} averaging).

Because these are individual sites and not zonal averages, this is not an area weighted trend. The error bars are 1 standard deviation.

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.

The OMI team appears to have done a good job, but it also appears that the row anomaly effect has not been properly considered. This does not affect total column ozone too much, but certainly effects the LER. There is a problem with the long-term trends from the OMI team product in that the correlations between ozone, aerosols, and cloud transmission seems incorrect (see previous partial reply)

The effect of absorbing aerosols has now been added in a manner now described in the revised paper. I have compared the current results with that from the OMI team at several sites that we both considered. See Table 1.

4- Bias of OMI erythemal data

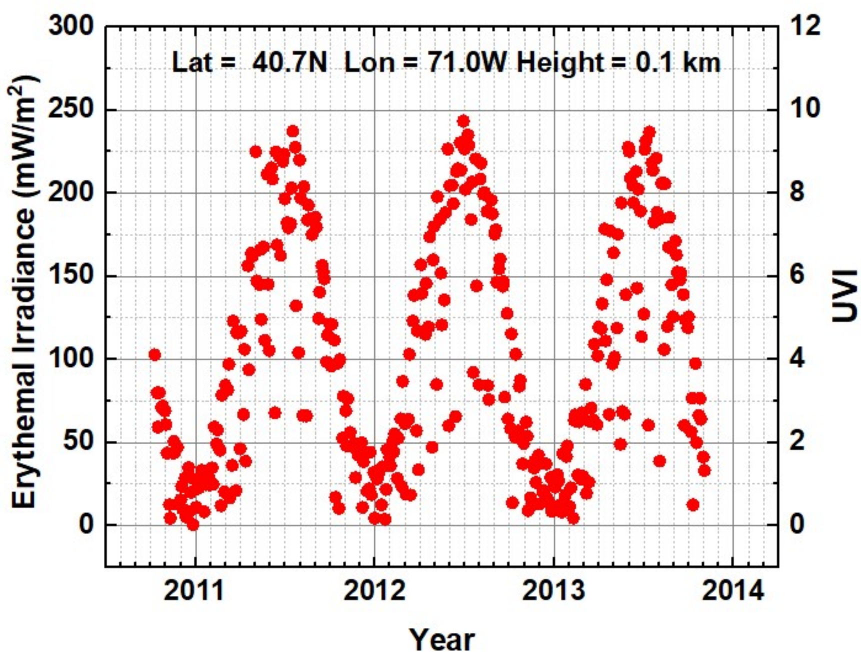
In general, the paper overstates the ability to accurately determine the UV irradiance at the surface with measurements from space, in particular in the presence of (absorbing) aerosols and clouds. As also stated by Referee #1, OMI UV data have been compared with ground-based measurements at many sites. 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).

I have added a paragraph on page 7

The site at Santiago, Chile shows a overestimation case where the effect of absorbing and scattering aerosols may not be properly taken into account in calculations using OMI data for a city located in a depression surrounded by complex high terrain (Cabrera et al., 2012). Calculations in this paper (see Table A4) show a maximum summer value of UVI = 14, when ground based measurements within the city show peak values near 12. The overestimate is consistent with calculations made previously using other satellite data.

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 (quanti?ed 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.

Unfortunately, the data reference in Fan et al., 2015 http://lllab.phy.stevens.edu/slco is no longer available for me to check cloudy conditions myself. However, I have included a comment on the results of this paper in the text. The relatively clear-sky values agree with Fan et al. as they discuss. I cannot tell from the plots in the paper vs my calculations with absorbing aerosols whether the agreement is better or worse. The OMI row anomaly limits the number of data points per year as shown in the graph below.



On Page 7 I have added the comment

Another analysis of erythemal irradiance estimates from OMI satellite data in the New York City area (Fan et al., 2015) found that the calculated UVI overestimates the measured UVI under cloudy conditions, a result that might affect estimated trends.

I also like to note that the effect of absorbing aerosols on the UVI is very dif?cult 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.

The absorbing aerosol optical depth has been determined from EPIC (Torres et al.) at 354 nm and scaled to 310 nm (see eqn 3) based on measurements made over Korea

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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.

I have compared the average erythemal amounts between the two data sets, which mean that the algorithm gives same answers within the error estimates. For a given city, EPIC will make measurements at 3 to 4 times per day, but not necessarily at the local time of the OMI overpass, which means that the cloud transmission is likely to vary. I have added the following text (page 20):

Computing the global and seasonal average E percent difference $100(E_{EPIC} - E_{OMI})/E_{EPIC} = 1.4 \pm 1\%$. In the presence of clouds, local differences may be larger, since the OMI latitudinal overpass GMT can vary by ± 20 minutes from the equator crossing GMT causing apparent changes in local cloud cover from the specific EPIC GMT t_O. Also, the OMI analysis contains an assumption that TCO_3 C_A, and C_T measured at 13:30±0:20 apply to the local noon erythemal calculation (SZA = Latitude – Solar declination).

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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.

T is now C_T SZA is now θ everywhere

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.

The paper is long, but the amount of text is not long. It is largely the figures that make the paper seem long, especially with the figures appearing twice, once in the text and once at the end. The paper is now clearly divided between OMI and EPIC, which should remove most of the repetition.

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**** Specific Comments

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

The ozone values between EPIC and OMI agree well, so I expect that the erythemal irradiance will also agree.

L21: Change "high northern latitudes" to "northern mid-latitudes" ("High latitudes" typically refers to 60? to 90?, not 40? to 60?. "high northern latitudes" is no longer is in the paper

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

Added two more recent references

L50: "skin. erythemal" > "skin. Erythemal" Corrected

L71: Please provide a reference to back up the assertion that OMI is "well calibrated" Done

L108: "data gaps" should also be mentioned here. There are data gaps in the time series shown that occur because of the row anomaly.

The time series depicted in Fig.3 are non-uniform in time with significant gaps, mostly from the row anomaly, between some adjacent points.

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.) OK

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). OK

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

L167: In the standard de?nition, the Radiation Ampli?cation 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 quanti?ed. (see Herman Use of an improved radiation amplification factor to estimate

the effect of total ozone changes on action spectrum weighted irradiances and an instrument response function JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 115, D23119, doi:10.1029/2010JD014317, 2010)

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. (The error is much smaller in the work in Herman, 2010)

L169: Please explain the term U(zeta) in Eq. (1). U(θ) is a fitting coefficient to the radiative transfer solution along with R(θ) (see Herman 2010)

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. Fixed everywhere

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

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.

The 90 points for 2005 in the former Fig4 Panel B (now Fig5) have no smoothing or averaging. The solid lines have now been changed to an Akima spline fit passing through all of the points.

On page 13:

Note that there are only 90 points in 2005 because of data gaps in OMI equatorial data and the effect of losing points because of the row anomaly.

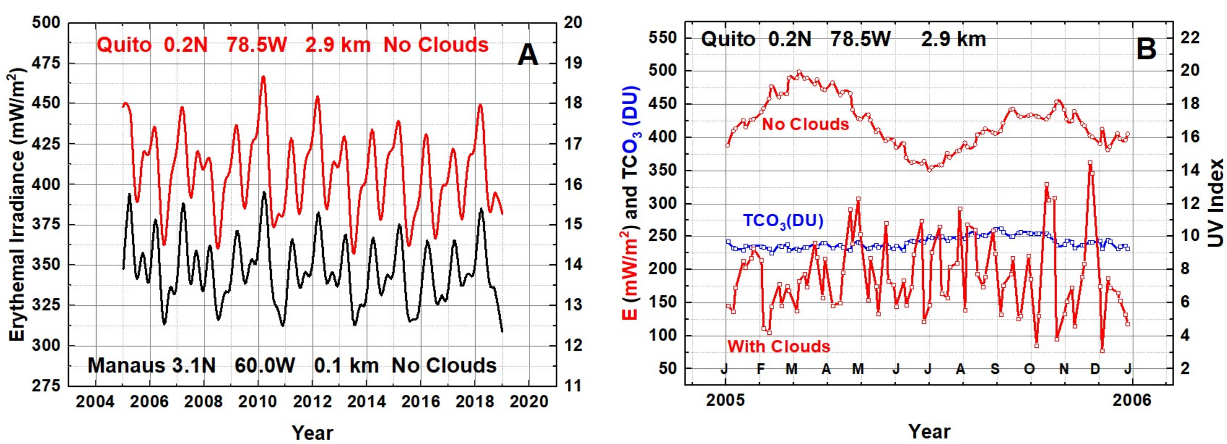


Fig. 5 Panel A: A two week running average of cloud-free E(ζ,ϕ,z,t) corresponding to the data in Fig. 4 for Quito Ecuador and Manaus Brazil showing the effect of height and a small difference in average

ozone amount. Panel B: A temporal expansion for one year (2005) of $E(\zeta,\phi,z,t)$ estimates for Quito showing the double peak as a function of minimum SZA near the equinoxes in the absence of clouds that is masked when clouds are included. The blue line shows the 20 DU variation in ozone between March and September. Plotted points have no averaging. Solid lines are an Akima spline fit.

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.

On page 14, the following sentence has been added

Occasionally, ground-based measurements show that UV irradiance at the ground can exceed the clear-sky value because of reflections from nearby clouds

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.

I have removed that statement. It was a matter of visual judgement and not good science.

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

The sentence now reads: The maxima occur close to the December solstice date, with the exact date shifted by cloud cover, and the minima occur near the June solstice date.

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! Fixed

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.

I have added text based on your comments on the ozone maximum at Ushuaia and referenced Bernhard et al. (2010). The paragraph now reads:

Previous estimations of erythemal irradiance from measurements (1997-1999) and calculations (using Total Ozone Mapping Spectrometer data) at Ushuaia (Cede et al., 2002; 2004) shows very similar values with UVI < 1 in the winter (June) and with 14-year average maximum values up to 8. The OMI data shoes an occasional point reaching 10 during the summer (December-January). The 20-year historical ground-based measurement record at Ushuaia starting in 1988 (Bernhard et al., 2010) shows higher values, 11.5, when the Antarctic ozone hole moved overhead in October even though the SZA is not a minimum. Buenos Aires at lower southern latitudes has values of UVI from 1-2 in the winter and up to 12-13 in the summer. These values approximately agree with those in Table 4

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.

The paragraph now reads:

Evaluating E(ζ,ϕ,z,t) over Antarctica from OMI data is likely not accurate because the reflectivity of the scene is approximately treated as if there were a thin cloud over a bright surface. The calculated transmission function C_T(ζ,ϕ,t) has a minimum of 0.89 resulting in a difference in E(ζ,ϕ,z,t) between setting $C_T = 1$ and using the Antarctic Peninsula calculated C_T(ζ=-70, ϕ=-64) of less than 10%. The annual cycle ranges from 0 in winter (May to August) to a variable maximum in the spring and summer months depending on the year. From the OMI data, for example, 125 mW/m^2 (UVI=5) in 2013 and 175 mW/m^2 (UVI=7) in2016. The year to year variation in the maximum E(ζ,ϕ,z,t) is driven the highly variable Antarctic ozone hole TCO_3.

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.)

Sentence changed slightly: In these images, local solar noon is near the center of the image, but offset by EPIC's orbital viewing angle that is 4^O to 15^O away from the Earth-sun line. In the case shown, the six-month orbit is offset about 10^O to the west. Three months earlier in March and three months later in September, the orbit is offset to the east.

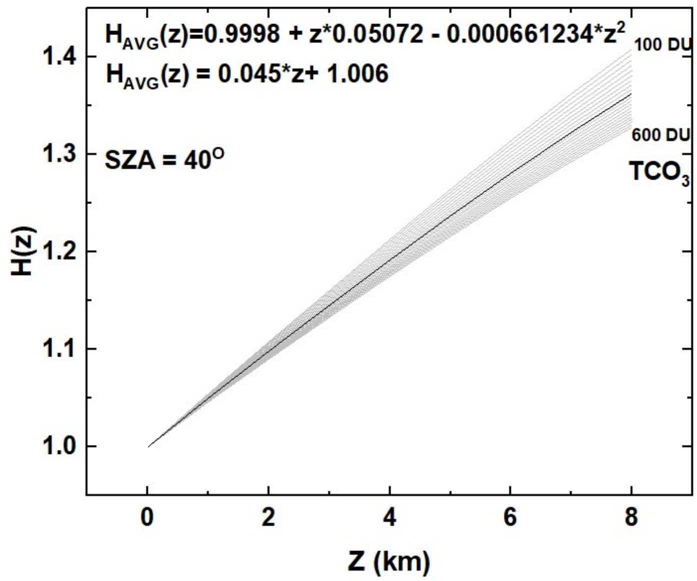
Figure 10B: I suggest to indicate the location of Mt. Everest in the ?gure. Figure is removed

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

The labels are 11A and 11B

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.

The reviewer is right. The RT program was run for altitudes 0 to 5 km, so Mt Everest's extrapolation to 8.8 km may not be accurate. I just ran the code for 8 km, which changed the fitting parameters slightly, but did not significantly change the average of 14 years of maximum value of UVI = 18 in Table A4. I added the sentence on Page 19:



Height Dependence H(z) 0 < z <8 for erythemal irradiance for a range of TCO_3 $100 < \Omega < 600$

The height dependence of UVI for Mt. Everest (8.8 km) is extrapolated from calculations for 0 to 5 km. Calculations to 8 km show that this is a good approximation.

From Table A3

Erythemal ERY	$1 + 0.047\ Z_{km}$

I have added validation of the maximum observed UVI in the US from ground based and estimated erythemal irradiance with the results shown in Table 2

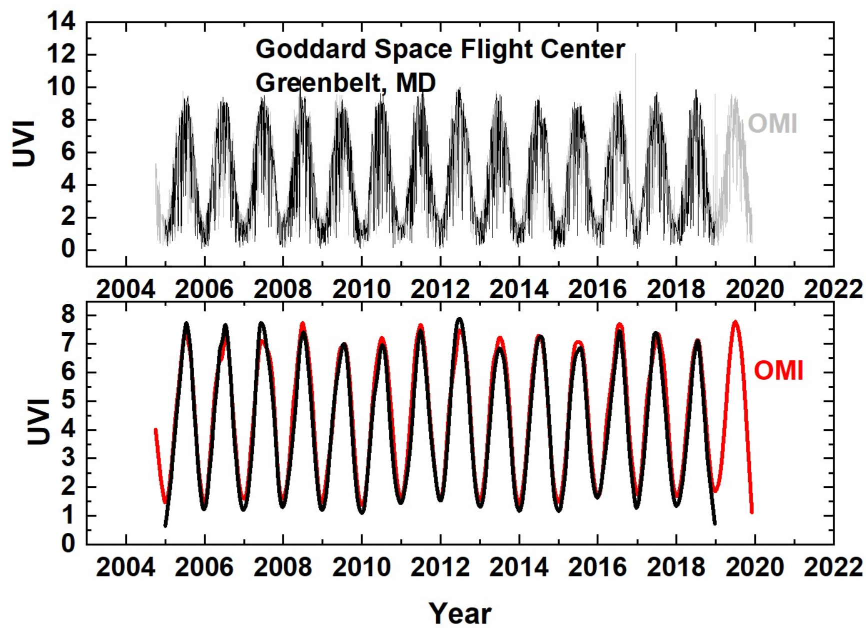
Table 2 Comparison of Calculated OMI UVI with Ground-based Measurements

Site	Ground-Based UVI June Maximum	Calculated UVI June Maximum	Latitude	Altitude Meters
Beltsville, Maryland[1]	10	10	39N	60
Lamar, Colorado[1]	11	11	38.1N	1104
Waimea, Hawaii[1]	**16**	**12**	22N	0
San Diego, California[2]	11	11	32.8N	9
Flagstaff, Colorado[1]	11	12	35.2N	2128
Griffin, Georgia[1]	10	11	33.2N	300
Houston, Texas[1]	11	11	29.8	0

[1]https://uvb.nrel.colostate.edu/UVB/da_Erythemal.jsf
[2]http://uv.biospherical.com/updates/boreal/euvindex.aspx

Table 2 shows a comparison of the June maximum UVI values estimated from OMI ozone and LER data with June ground-based measurement data. June was selected for the comparisons, since the SZA changes slowly near solstice permitting at least a week's data to be considered selecting a maximum that can be compared with comparable calculated Erythemal irradiance calculated from OMI data.The day-to-day measured and calculated variation at solar noon during June is greater than 10% for nearly clear-sky days. Except for Waimea, Hawaii, the agreement is quite good. These are typical maximum values that vary slightly year-to-year. A similar validated (Tanskanen, et a., 2006; 2007) OMI erythemal data set is available for many sites https://avdc.gsfc.nasa.gov/index.php?site=2057856112&id=79 that uses the same OMI ozone data. The current erythemal data set is more strictly filtered for the row anomaly.

A comparison of the two data sets is shown below with the AVDC OMI data set labelled (grey and red)



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 caption is now:
Fig. 15 Longitudinal slices of UVI(ζ,ϕ,t_O) at 0.1^{O}N and 30.85^{O}N latitude indicated by the short dark horizontal bars in the two color images. The EPIC E(ζ,ϕ,t_O) (mW/m^2) images are for 14 April 2016 t_O = 04:21 GMT centered at about 10^{O}N and 104^{O}E. Panels A and C show longitudinal slices of E(ζ,ϕ,t_O) and C_T(ζ,ϕ,t_O) for ζ = 0.1^{O}N and panels B and D for 30.85^{O}N. The solid lines in panels A and B represent the SZA.

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.

This has now been totally redone (see pages 29 and 30)

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.

This has been done The zonal averages are now in the EPICsection

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.

Bad labelling of the figure. The figure is the zonal average of the maxima, not the zonal maximum. The text states that clearly: "The zonal average maximum (Fig. 17A) of about UVI = 14 is approximately the same for any day of the year. This includes longitudes containing high altitude sites at moderately low latitudes where the local UVI maximum can reach 18 to 20.". I have fixed the figure caption and label.

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 14B in the original manuscript (now figure 7B) there are no error bars. Figure 7A does have error bars that are independently determined for the time series corresponding to each site. The variability is mostly driven by changes in cloud and aerosol transmission, C_A and C_T and can differ from site to site.

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.

Figure 17 in the original manuscript has been removed

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

Dangerously has been replaced with extremely. However, the word dangerous has been retained in one sentence:

". The EPIC and OMI observations show that there are the wide areas between 20^O and 30^OS latitude during the summer solstice in Australia (Fig.12) showing near noon values with UVI = 14, values that are **dangerous** for production of skin cancer and eye cataracts and correlate with Australian National Institute of Health and Welfare cancer incidence health statistics (2016). "

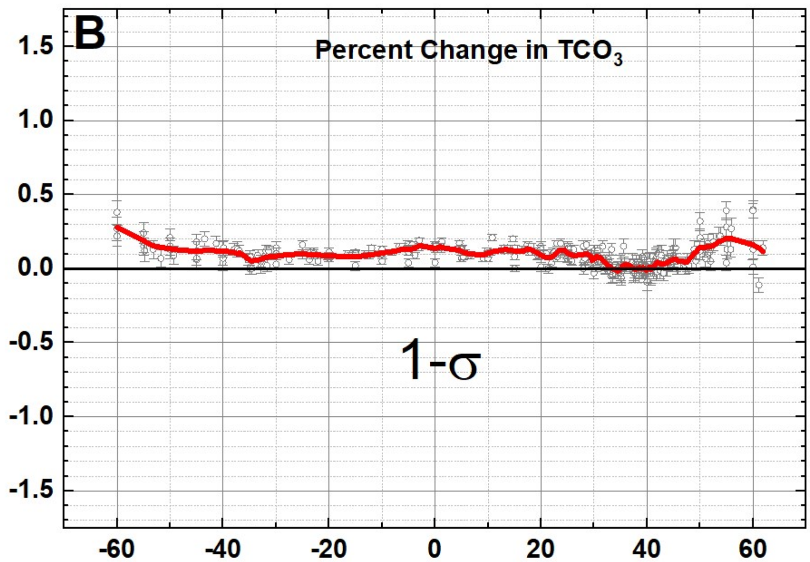
L549: The conclusion that "nearly half the sites have shown 2-sigma changes in UVI" may need modification if the drifts of OMI mentioned earlier turn out to be true or if the simplifications of the regression analysis also discussed earlier resulted in spurious significant trends.

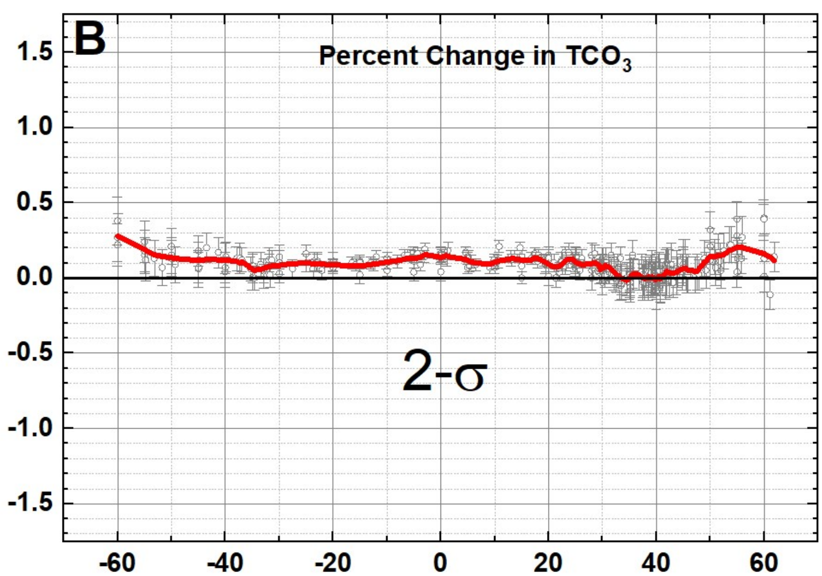
The recalculation of the trends has changes the conclusion so that less than half the sites now have 2-

sigma changes. Accordingly, the sentence on page 31 has been changed to "However, a significant fraction of the sites has shown 2σ changes in….."

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.

Ozone now shows changes at the 1-sigma level at most latitudes, but not at the 2-sigma level





There are significant changes at low latitudes (25S to 20N) and at high latitudes

The sentence now reads: "The increase is partially offset by TCO_3 showing significant latitudinal increase at the 2-σ level between 25^{O}S to 20^{O}N and at high latitudes that only affects UV wavelengths

(300 – 340 nm)”

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. Fixed

Erythemal irradiance $E_0(\theta, \Omega, C_T)$ at the Earth’s sea level (W/m^2) is defined in terms of a wavelength dependent weighted integral over a specified weighting function $A(\lambda)$ times the incident diffuse plus direct solar irradiance $I(\lambda,\theta,\Omega,C_T)$ $W/(nm\ m^2)$ (Eq. A1).

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.

Please read the Herman 2010 paper. The formulation is of RAF is different

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

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.

$H(\theta,\Omega,Z)$ scales the erythemal irradiance at the surface to an altitude z and was calculated by fitting a function to the ratio $R_E = E(\theta,\Omega,z)/E_0(\theta,\Omega,0)$ where E and E_0 were calculated with the TUV radiative transfer program. Most of the θ and Ω dependence is derived from $E_0(\theta,\Omega)$.

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.

It may seem strange that double precision is needed, but I tested the expressions at single precision and the accuracy of fit is degraded in some cases. There are single precision expressions that work fairly well, but not as well as the rational fractions at double precision. Since the numbers can be copied from the pdf, there is no reason not to use the double precision in a modern computer. Plus, there is a little-known anomaly in modern computers. Double precision is faster than single precision because the arithmetic portion of the chip is always double precision and an extra step has to be taken to convert it to single precision.

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). My mistake…..95% is the right number

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

Now reads: "Fig. A1 Values of the function coefficients $R_{ERY}(\theta)$ and $U_{ERY}(\theta)$

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. ———-

**** Technical Corrections:

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

L28: There should be two closing brackets after "(3.78 km)" to match the opening bracket start at "(e.g.," (e.g., San Pedro, Chile, 2.45 km; La Paz, Bolivia, 3.78 km).

L69: "latitude dependent" > "latitude-dependent" OK

L86: "occasionally a 2nd" > "occasionally 2nd" OK

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.")

The numerical algorithm for erythemal analysis is applied for the Northern and Southern Hemispheres and the equatorial region is discussed in the Appendix of this paper.

L260: "a lowest" > "the lowest" OK

L313: Fig. 6, > Fig. 6B, Not there any more

L315: The part of the sentence starting with "resulting in a difference in . . ." sound very awkward. Please improve! The sentence is unnecessary and has been removed

L404: "ae quite" > "are quite" Not there anymore

L486: "Atmospheric Transmission," should be lower case OK

**** References OK

Bernhard G., C. R. Booth, and J. C. Ehramjian, Climatology of Ultraviolet Radiation at High Latitudes Derived from Measurements of the National Science Foundation's Ultraviolet Spectral Irradiance Monitoring Network, in: UV Radiation in Global Climate Change: Measurements, Modeling and Effects on Ecosystems, edited by W. Gao, D. L. Schmoldt, and J. R. Slusser, 544 pp., Tsinghua University Press, Beijing and Springer, New York, 2010. https://link.springer.com/chapter/10.1007/978-3-642-03313-1_3

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Global Atmosphere Watch, Report No. 164, WMO TD-No. 1289 (Geneva), 55, 2006, available at: https://library.wmo.int/doc_num.php?explnum_id=9302.

WMO (World Meteorological Organization), Scientific Assessment of Ozone Depletion: 2018, Global Ozone Research and Monitoring Project–Report No. 58, 588 pp., Geneva, Switzerland, 2018.

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 #3
Received and published: 13 November 2019

Yellow color is my reply to the reviewer. Grey color represents text added to the manuscript

The manuscript by Herman et al., "Global Distribution and 14-Year Changes in Erythemal Irradiance, UV Atmospheric Transmission, and Total Column Ozone 2005–2018 Estimated from OMI and EPIC Observations" presents a study of estimated surface UV and 14-year trends from Ozone OMI time series, as well as UV estimates utilizing measurements from the Earth Polychromatic Imaging Camera (EPIC). The topic of this manuscript is relevant and interesting and suitable for the scope of the journal. However, I see many areas where the weaknesses and uncertainties of the applied methodology were not properly discussed and addressed. I consider it takes a major revision, before the paper is modified and revised to the form, which can be accepted.
I strongly agree with the other reviewers and do not repeat those points all here. However, I do want to further stress few points particularly in the evaluation of the Anonymous Referee #2.

Absorbing aerosols. In this methodology no effort is done to account for that effect. However, it is a strong source for potential bias in satellite-based surface UV. And it can be a strong and wrong source also for the trend estimate, since any real trend in absorbing aerosols shows up as an erroneous trend in surface UV. And absorbing aerosols make a two-fold effect. Increasing absorption as such means a reduced level in surface UV, which this method does not take into account at all. But this absorption effect results additionally high-biased cloud modification factor, CT. In case of increasing fraction of absorption, for a given AOD, the TOA reflectance decreases, which in the current method means higher CT value and thus higher surface UV. Unfortunately, this impact is then just opposite to the true impact of increased aerosol absorption in the surface level UV.

The effect of absorbing aerosols is now included based on the work of Torres et al. (see revised paper). The paper has been extensively revised and reorganized so that marking individual changes in the text is not feasible. Most of the changes are listed in detail in response to reviewer #2.

So the above reasoning makes the reader wonder how much there is this effect involved for instance in the Figure 18. By the way, I assumed there was a typo, so it should be Russia-Indonesia (not India) and not 120W, but 120E. Is this right?
There was a typo as the referee stated. However, the old Figure 18 is no longer in the paper

There are typically very strong fires in Indonesia (and peat fires are particularly strongly absorbing at UV, while there is not much absorption at visible) and also discussion about the long-term trends in the fires activity. So, there should be some discussion about these effects (if those regions were included at all in the analysis).

Absorbing aerosols are now included

In addition to the absorbing aerosols, it was surprising that nothing was said about areas of potential "snow contamination" in the estimated UV. If -60 to 60 latitudes are included, there are still large areas of seasonal snow cover. Moreover, these are also regions of likely trends in this snow cover. About both aspects, Bormann et al. 2018 is illustrative, there are significant regions within -60 to 60 with seasonal spring time snow cover variability and trends have been also detected of snow melt occurring earlier.

Based on what you wrote, one would assume that you used surface reflectivity of 0.05 and same constant everywhere (although it was not stated explicitly).

The spatially resolved reflectivity climatology data set R_G used was derived from TOMS data (Herman and Celarier, 1997) for snow/ice free conditions (this is stated on page 20 of the revised manuscript and was in the original). However, I found that use of a nominal single average reflectivity $R_G = 0.05$ for snow/ice free conditions makes little difference in the time series (i.e., well within the error bars for trend estimates) in the results and trends. Surface reflectivity in the UV is small almost everywhere (most land, oceans, and vegetation). An exception are some desert regions in Libya, where small regions can reach 0.1. White Sands National Park in the US (gypsum sand) is another small region where R_G is larger than usual. This paper is a study of cities, where these exceptional conditions do not apply.

Then, over snow covered regions, this means the satellite measured "excess" reflectivity due to the high snow reflectivity in reality, is put erroneously to the cloud attenuation (meaning too low CT value). Similar to the problem of absorbing aerosols, this has now double effect. Higher surface reflectivity should result in higher surface UV due to the surface reflectivity alone.

High surface reflectivity exists only briefly in cities after freshly fallen snow. Automobile and pedestrian traffic quickly reduces the reflectivity as does the presence of tall buildings.

But in your method, the surface reflectivity (enhanced by snow) is not considered and moreover too strong cloud attenuation is assumed, both aspects contributing to the too low surface UV. This means that there are regionally large biases in the estimates surface UV, but perhaps even more importantly that there can be large artificial biases and errors in the trend estimates too. These things should be considered (or at least discussed thoroughly).

Most of the cities considered in this study have little or no snow as a long-term feature. For those that have considerable snow, the UV is underestimated on clear-sky days at noontime.

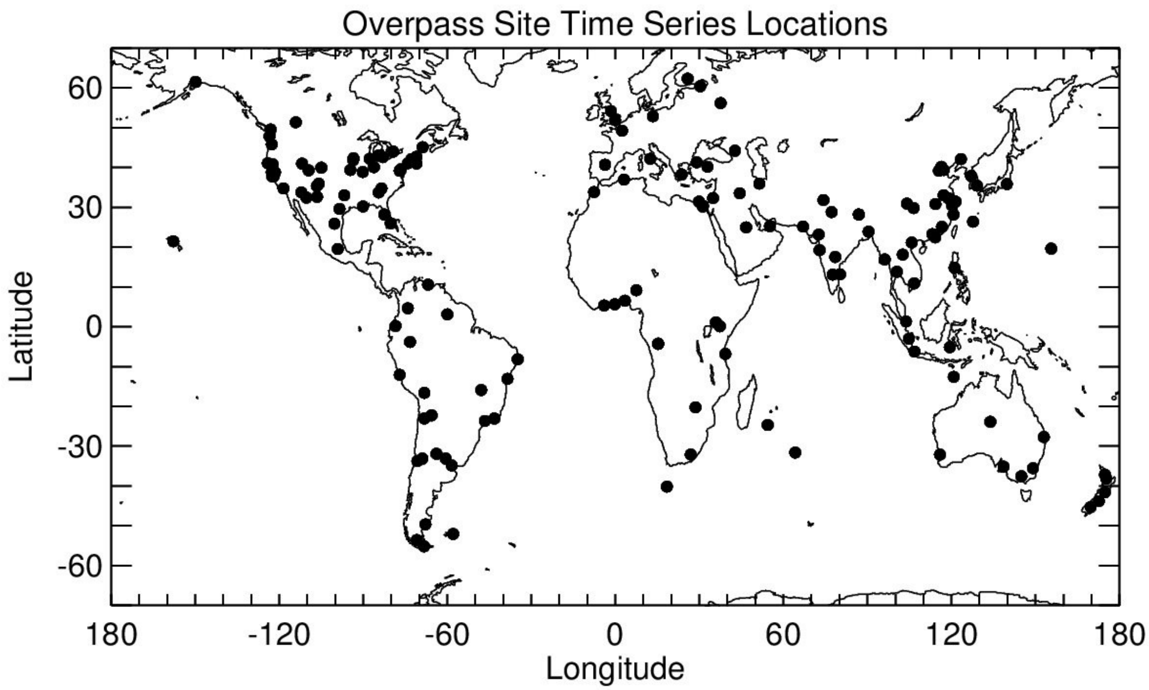

Fig. A3 Map of locations in Table A4

Some, like Moscow, Russia or Helsinki, Finland have considerable snow in the winter, but none in the summer months when erythemal irradiance is a maximum. Winter in these cities also has persistent heavy cloud cover on most days, which minimizes the neglect of snow reflectivity. The current study does not take into account the effect of snow cover for the considered latitude range. The effect is that erythemal irradiance is underestimated in winter conditions for high latitude cities usually when the SZA is large and erythemal irradiance is small due to atmospheric absorption and scattering.

The following sentence has been added on page 4:

The effect of snow and ice on the surface reflectivity during winter months has been ignored in this study. This means that the already low amounts of erythemal irradiance during winter in high latitude cities because of high solar zenith angles is further underestimated in the presence of snow and ice.

Bormann, K.J., Brown, R.D., Derksen, C. et al. Estimating snow-cover trends from space. Nature Clim Change 8, 924–928 (2018) doi:10.1038/s41558-018-0318-3