Response to Co-Editor comments:

Comment on "Relationship between erythema effective UV radiant exposure, total ozone and cloud cover in southern England UK: 1991–2015" by Nezahat Hunter et al.

We welcome the Co-Editor's comments. Here are our replies to the issues raised. The manuscript has been changed throughout.

Please note that one of my colleagues surname has changed to "Rebecca. J. Rendell".

Based on the answer to the reviewers and the new document, I still have some major issues with major aspects of the used methodology and the interpretation of the results. I have got additional comments from one of the reviewers based on the new text and I have combined them with my comments that can be found below.

In long term series studies three major aspects have to be clear in order to ensure valuable Results

- The data quality and the related uncertainty based on the quality assurance, quality control and calibration procedures that have been followed

The data quality of the UV radiant exposure data is clearly beyond the scope of this paper. Regarding data quality and uncertainties, these have already been discussed in the following publications by Hooke et al. (2018) and Hooke et al. (2017). Further brief explanations have been also made in the text to clarify specific issues.

Detailed description about detectors and total ozone measurements in the Camborne and Reading sites have already been introduced in the published study by Smedley et al (2012). We have also investigated satellite-based total ozone data from the OMI data products in Reading and also in Chilton for the period (2005-2015). Both ground and satellite-based datasets were compared and the results revealed that the ground-based data in Reading are almost identical to those values from the satellite-based measurements in Reading and Chilton, but these results are not shown in the manuscript.

Cloud cover data have been collected together with other meteorological variables by the Met Office Hadley Centre in the UK and a detail description of the data has already been published elsewhere (Dunn et al. 2012 & 2014). The data were obtained from the Centre for Environmental Data Analysis (CEDA). The detailed information about data quality, uncertainties and calibration procedure in cloud cover measurements, again are outside the scope of this paper.

- The methodology of treating/averaging/checking the datasets

As a trained statistician with many years' experience, I believe the methodology and data analyses carried out here have followed the correct statistical procedures.

- The interpretation of the results and the derived changes (here in UV) based on presented changes of the factors affecting UV.

The text has been revised to address these points throughout.

More specific:

Abstract

I think that the included sentences such as: "All these changes are small and occur within a very variable signal." Plus the fact that is difficult to determine e.g. 1% changes with instrumentation that is just "better than 10%" have to be included here.

The abstract could be much more clear I suggest deleting the whole section starting with the new RAF text on line 19 up to line 27 and just continue with the last paragraph of new text (plus the comment above).

The Abstract has been revised in line with some of your suggestions. However, we have not included the comment above (highlighted). We agree that all these changes are small and it has been well understood that changes in total ozone, cloud cover and AOD effect the total erythema effective UV radiant exposure (H_{er}). These factors only contributed half of the changes in the H_{er} in our study while the other half is due to other climate variables that are difficult to predict due to continuous changes in these factors from day-today and year-to-year and that climate change are also effecting these changes. All these factors also vary depending on location, study period selected etc.

83 UV data

What was answered in one reviewer about not having gaps in the UV data series have to be included when describing the UV data section.
The previous reviewer response did not say that the UV dataset had no gaps but rather that the number of missing days was small (3%). This has been now added into manuscript for clarification (see Line 118-121).

It is amazing 25 years of UV data without gaps. Is the spectroradiometer calibration performed on site ?
Yes the spectroradiometer calibration is performed on site _ the spectroradiameter input optics are located close to the Chilton broadband detectors. The word "co-located" has been added in to the paragraph explaining the calibration method.

Reading the argument from lines 100-105 it shows that in the second half of the 25 year period the max drift there could have been is 20% (+10% to -10%). The uncertainty in the data for all weather conditions and across the complete time period should be provided.

Historically the Chilton head has been calibrated annually against a reference spectroradiometer which has been calibrated traceably to national standards. This principle of calibration has not changed throughout the period these data have been collected. Our consideration is that overall the uncertainties in the data are similar throughout the 25 year period, perhaps with an increase in uncertainty at the start of the dataset.

Also, after annual calibration, was data corrected if a change in the instrument was seen, and if so how? By linear interpolation back to the previous calibration, or some other method, or was the calibration only corrected going forward?

The new calibration was applied going forward and without linearly interpolating back to the previous calibration.

107 Ozone

Ozone data could be continuous but Dobson and Brewer monitoring schedule/data availability depends on solar zenith angle and mostly cloud conditions. So some details on these data will help the reader to understand the use of the time series.
Some extra text has been added in this section 2.2 (see Line 137-140).

141 cloud averaging

A use of a constant 3 hour/day averaging of cloud coverage for assessing their impact on daily solar UV changes includes the uncertainty related with the cloud changes in the remaining daytime. This should be mentioned. A more realistic solution would be to weight the cloud amount for every hour with the percentage of cloudless sky UV for the specific hour versus daily cloudless UV. However, as this requires a lot of additional work the introduced uncertainty can just be mentioned.

We believe averaging cloud cover for 3 hours/day (11-2pm) is appropriate to use here given that the UV measurements during this time contribute a large proportion of the daily H_{er} overall. The text has been added to clarify this (Line 167-168)

144 Statistics

There is something that I cannot understand statistically. How is it possible in a time series with no gaps the Hooke results that are based on summing up all days for a year and then calculate yearly anomalies and trends to be different than averaging daily values for each month and then calculating monthly anomalies and then trends. My impression is that the sum of the monthly anomalies in a year has to match the yearly anomaly calculated by Hook for the same year (with very small differences due to the small differences of the number of days in a month).

The UV data has only 3% of missing days which have been treated here as missing values in the analysis and statistical analysis excludes them. However, in our previous analysis by Hooke et al. (2018), these missing days were filled in with the average value for each day over the entire period. There may therefore be some small differences between two analyses. We have added a text to clarify this (Line 118-121).

Trend estimate should be the same whether you consider annual mean, monthly mean, annual anomalies or monthly anomalies. However, the trend between our previous published study and here is different mainly because chosen calculation period is different for both papers. Please also see Line 573-587.

Outliers

More information on objective algorithms on rejecting outliers from the analysis should be provided.

We have not used any algorithms on rejecting outliers from the analysis. We did not exclude outliers neither from the dataset nor from the analysis.

The new text (lines 213-216) is contrary to the objective of the paper. If looking for underlying reasons for a trend (ozone, cloud) then data that may be particularly high/low due to ozone or cloud should not be removed from the dataset. They can be removed it if instrument problems have been identified. Otherwise the outliers seem arbitrary, and how can data be an outlier one year when it is well within the whisker value for another year?

Nowhere in the manuscript does it say that outliers were excluded in the analysis.

Ozone and UV seasonal trends

For the second period where clouds are almost constant and ozone plays the only role in the UV trends: the same amount of ozone changes has different effect in the UV for different seasons due to the differences in the related air masses. More specific constant ozone trends during all seasons would theoretically lead to higher UV trends in winter than in the summer months (in percent). From tables 2 and 3 seems that the opposite was found. Is there any explanation for this ?

By looking at table 2 and 3, we agree that UV trend in winter is lower than that in the summer (in %) for the second period. However, when the ozone trend values in winter (0.66% y^{-1}) and summer (0.13% y^{-1}) for the second period in Table 3 were multiplied with the RAF values in Table 4 (-1.66 and -2.18 respectively), the observed increase trend in total ozone for the second period corresponds to higher trend of -1.1% y^{-1} in winter in H_{er} and for summer it corresponds to the trend of -0.28%y^{-1} in H_{er}.

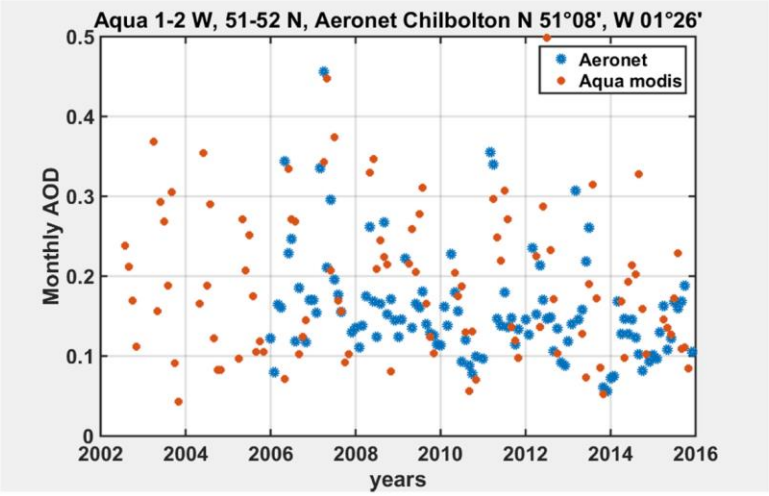
Aerosols

The atmospheric related reasons for UV changes can be clouds, ozone, aerosol optical depth, other aerosol optical parameters, albedo, other traces gases, … (more or less ranked here based on their importance) changes. So if there are no data other than clouds and ozone you have to clarify it.
Done, see Line 181-183.

For the case of aerosols Zerefos et al., 2012 as you mention, presented changes also in non urban areas. I think that Aeronet/Chilbolton data and satellite data can be used in order to provide a hint on current speculations about a negligible 25 year effect of aerosols on this time series.

I could fast download 1 by 1 degree data around Chilton form MODIS Aqua shown below.
In addition Aeronet level 1.5 from Chilbolton station.
Modis/Aqua shows an AOD decreasing trend in the order of ~-0.003 or ~-1.5% per year. And Aeronet ~-0.007 or ~3% change per year.

This figure is a bit rough in terms of spatial resolution for Modis/Aqua and the data have been just plotted as they are with no checks at all. But some work on this aspect (e.g. lower spatial resolution Modis data or just use of the Aeronet data) can provide some more insights on the aerosol issue for the second period. Aeronet 1.5 level data also I just plotted the NASA site existing monthly mean data. They represent Chilbolton station/area.



If you end up on similar results then more or less results for this period agree with Zerefos et al., 2012 that state that there is a turning point that (for constant cloudiness) that the ozone increase masks the aerosol (slower rate than before) decrease for mid-latitudes.

Thank you for this useful information which is very helpful. However, the last paragraph (highlighted yellow) is not very clear to me.

We did not considered aerosols here because no ground based AOD data over the full period (1991-2015) were available near the Chilton site. However, we have now revised the manuscript a fair amount by adding new sections regarding aerosols data. We have used the AERONET dataset to analyse aerosol optical depth AOD from the Chilbolton station and comared with H_{er}. Please see sections 2.4, 3.5, 3.6, 4.4 and 4.5.

Figures

The quality of the figure 3b in the paper is not good it needs some improvement on the submitted figure format.
The fig. 3b has been redone.

I would suggest to move some of the sections describing previous works related with UV vs clouds, ozone, aerosols in the introduction section and summarize the agreement/disagreement etc findings of your work compared with the mentioned publications in the last section.
As suggested, some of the text from the discussion has moved to the introduction section (see Line 78-89).

One of the native English speakers on the author list should go through the new version of the manuscript and correct the grammar – particularly, but not limited to, the new text.
Done.