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Atmospheric Chemistry and Physics Discussions

Interactive 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.

Anonymous Referee #2

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This manuscript explores the changes in erythema effective UV radiant exposure over a 25 year period, and the associated changes in total ozone and cloud cover that might be expected to influence UV radiation at the ground. This is a significant time series for ground-based UV radiation measurements and as such the results are instructive. The ozone and cloud cover data have been taken from longer datasets for stations relatively close to Chilton, the location for the UV measurements. The work is well presented but appears as a statistical exercise somewhat lacking in atmospheric interpretation. It raises a number of queries that must be addressed before publication of a final paper.

Section 2.1

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The previous paragraph states that monthly UV doses are considered in the manuscript. Section 2.1 then details how a daily dose was calculated. Please specify how a monthly dose was then determined – is it the sum of all days in the month, or the average of all days in the month (that is it becomes a mean daily dose for the month). How was missing data treated? Was there a limit to the number of missing hours allowed for calculation of a daily dose, and similarly what were restrictions on missing days in determining a monthly dose? The same questions apply to the external datasets that have been used for ozone and cloud cover. What were the minimum number of years that contributed to the overall monthly average for each of the 3 data sets?

Please provide a brief statement on the traceability and stability of calibration of the radiometers over the 25 year period. What is the associated uncertainty in the measurements and how can you be sure that there has been no drift, short- or long-term, in the measurement system?

Section 2.3 Seasonal variations have been removed from the data, but have longer term cycles been considered e.g. QBO and 11 (or 22) year solar cycle?

Please explain, or at least reference, the statistical techniques used (DW, MK, SS).

Section 3.1 Figure 1 – how were 'outliers' identified? In all seasons except winter the outliers from one year are clearly within the bounds of acceptable data for other years, so why have these data points been excluded? If they were beyond possibility for the site then there would be good reason to exclude the points, but this is not the case. In winter there are a large number of outliers – how did you determine that these data were unreliable? Please provide a clear justification for removing what appear to be valid data points from the analysis.

Define seasons i.e. which months have been used as 'winter'

Section 3.2 The annual ozone cycle is as one would expect at these latitudes. Com-

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ment on this and causes of e.g. low ozone events / particular occurrences e.g. in 2011. Note summer ozone (when UV is high) has very small and non-significant trends over any time period. The significant ozone trends in winter will influence the very low UV doses at that time of year, but have little practical influence on overall annual dose of UV. This fact is somewhat lost in dealing only in percentage deviations from average, where the winter % has the same weight as the summer %. Further comments on the implications for absolute UV doses are needed throughout.

Figure 3 – again please justify 'outliers'.

Fig 3b – what are the black line and the grey dashed line? The latter is not the mean value, as described in the text.

Section 3.3 Line 300 – comment on this with respect to Radiation Amplification Factors. Also comment on why RAF apparently changes with season or with period considered.

Section 3.5 Line 366-7 – qualify this statement, it is not necessarily a global truth. Also further down the paragraph you show that for a 1% change in cloud or ozone the response in H is greater for ozone.

Section 4 Lines 430 – 444 This does not produce a convincing argument for the analysis in this manuscript vs that of the previous publication. Both are described as 'best/better described by two linear trends'. Since both works use the same data set, how can the two linear trend selections be so different in the pivot point used to change from one trend to the next? This needs further justification. The overall change (full data set) should be the same for both analyses since the underlying data is the same. Is this the case?

Section 4.4 – discussion on aerosols. This is rather inconclusive. If AOD has been stable at Chilton then changes in aerosol/pollution cannot explain any changes in H. What is left as an explanation?

Lines 642-8 This (and the similar paragraph in the abstract) is almost counter-intuitive

in trying to manufacture associations between small changes in H, ozone and cloud cover. 1991-2004 has increased H associated with decreased cloud and no significant change in ozone (section 4, the abstract says there is an upward trend in ozone). 2004 – 2015: section 4 says there is a slowdown in the upward trend in H, and in the next sentence says there is a significant decrease in H. Both cannot be correct. The abstract only mentions a decrease in H. This is associated with a marginal upward trend in ozone and no significant change in cloud.

The abstract and discussion should be made consistent with each other. The abstract implies that both increasing and decreasing H occur at the same time as increasing ozone, but increasing H is more strongly linked to reductions in cloud cover, while there is no significant change in cloud over the period that H is reducing. Added to which all changes are small and occur within a very variable signal. Such a comment in the abstract, that all changes are small and some are not statistically significant, seems necessary.

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