Full review of the paper Relations between erythemal UV dose, global solar radiation, total ozone column and aerosol optical depth at Uccle, Belgium, by V. De Bock, H. De Backer, R. Van Malderen, A. Mangold, and A. Delcloo.

The paper is a relevant contribution to the knowledge of our atmosphere under change and most suitable for publishing at ACP. The data are unpublished before and the analysis methods are comprehensive. As a detail, the introduction of the extreme values and the frequency distributions on p.12 is clever. The results are presented in a straightforward, logical, and most interesting way.

The use of the measured data is mostly sound but leaves space a few comments as follows.

- On p.11, line 1-2: "Monthly means are only calculated for months with at least 10 individual daily values". This leaves the question why the minimum is 10 days and why not 15, 20, or 25, and what the effect is of accepting a monthly mean with no more than 10 or 15 days of data, eventually only covering the first or the very last part of the month while the long term monthly mean used in calculating the anomaly probably represents the entire month. During a month of a large change in solar elevation or other factors the lack of up to 66% of the days may cause a bias. Is it beyond the scope of this work to study the added uncertainty due to the missing days?
- P.15, Ch.4.1.1. leaves many questions. It may be unclear for the reader firstly why a linear trend can cause a change point in the time series, and secondly why the instrument was not calibrated in early 1998 although Ch.2.1. suggests that it was calibrated every month.
- On p.16 the sentence "No ozone calibrations were performed around 1998, so the change point has no known instrumental cause" is confusing. Can the instrument not drift or change by itself and, if not, then why is any calibration ever needed? A calibration, and not the lack of it as suggested in the text, in general would ensure that the data are fine. The abrupt change seen if Fig.3 would suggest an instrumental change because our first assumption is that the atmosphere only changes slowly. Are there any further facts telling that the instrumental change can be ruled out?
- P.19-20, Ch. 4.3.1. is discussing the trends in UV observed at other sites. Here the recent work by Eleftheratos et al (2014) could be included if relevant. However, the concept of "UV" remains unclear in this chapter. Probably it is not the same as in the analysis of data from Uccle, i.e. the daily or monthly dose of erythemal irradiance. If this is the case, then you could discuss whether the different trends listed are truly comparable. Perhaps one or two observations per day at SZA = 60 or 65 degrees do not represent the daily sum. Or do they, in a trend analysis? You may suggest this problem to be analyzed in a later study.
- On p.23 and on, Ch. 4.4. it is to be remembered that in Eq.(6) the S_g was derived from 10-minute and 30-minute data. The resulting modelled erythemal daily dose then has a much better time resolution than the measured UV dose. If the time resolutions were the same, the regression should probably be better. The largest outliers in the lower panel of Fig. 8 are likely to be a result of varying cloudiness that is poorly monitored by the Brewer. In your future work you may want to experiment by re-sampling S_g for the times of the Brewer UV scans only to get a better correlation coefficient than 0.96 (p.24, line 6). Also cf. the discusion by den Outer et al, 2005: UV radiation in the Netherlands: Assessing long-term

variability and trends in relation to ozone and clouds. J.Geoph.Res., 110, D02203, doi:10.1029/2004JD004824 (2005).

- On p.15, Ch. 4.1.2., please, state whether the trend in global solar radiation was positive or negative, and give the value, too.

Substantial conclusions are mostly reached but may require the following comment.

- The measurements at Uccle started at about the same time as Mt. Pinatubo erupted. What is its expected effect on the time series? To what extent does the observed recovery of ozone actually show the return to the stratosphere of the pre-Pinatubo time and to what extent the influence of the regulations of the Montréal Protocol? If this further analysis is beyond the limits of this work, it could be mentioned both in the analysis and in the conclusions (p.28), perhaps in the abstract, too.
- On p.18, line 5-8, the finding that the minimum values of global solar radiation have a large trend is most interesting. The conclusion "...this could mean that the cloud properties (such as cloud optical depth) changed over the past 23 years" may be too careful. Instead, you could probably say that "the cloud properties, i.e. their amount and/or water content, must have changed". The last sentence of the chapter "However, this is difficult to prove without direct information or measurements on cloud amount and/or properties" could be removed.
- On p.30 you quite right state that "What is seen in reality (i.e. an increase in erythemal UV dose accompanied with an increase in TOC and a decrease in AOD) is not always what is represented by the models". The significance of this sentence can hardly be overemphasized and should be brought into the abstract, too.
- On p.30 the discussion on which of the three independent parameters shall be included in the regression model does not sufficiently underline the fact that the regression is valid for one site, and perhaps one period of time, only. Moving it to another place or time is probably less hazardous if all the three parameters are included.

The language is probably fine but reading the text suffers from the excessive use of parentheses. Please, consider opening them as much as possible or just leaving out in case of self-evident or inessential information. The following suggestions are made:

- On p.2, line 8, the words “(without any known instrumental cause)” is something we all expect as a default and need not be mentioned.
- As always, the text could be more compact. E.g. on p.3 it says "Including TOC however, is justified as the adjusted R2 increases and the MABE of the model decreases compared to a model where only global solar radiation is used as explanatory variable" while it could be put shorter :”Including TOC however, is justified to increase the adjusted R2 and to decrease the MABE of the model".
- On p.5, line 20, please, replace"for a long time period of 23 years" by "for a time period of 23 years".
- On p.6. the first paragraph may not be needed in this detail. It could be sufficient to state "The cloud screening algorithm (De Bock et al., 2010) was improved by making use of the sunshine duration data and by assuming that the variability of the AOD..."
- Please, correct the title of Ch. 4.4.1. similar to that of Ch. 4.4.2., i.e. without abbreviations.

- Moreover, in several places the use of two different symbols for one physical quantity may be confusing. To be logical you may want to use one symbol for each quantity and replace TOC by Q_{O3}, AOD by τ_{aer}, etc. throughout the text.
- The text is scientifically sound except for one mishap in the sentence on p.4, lines 12-14 saying "In principle, long term trends in UV irradiance can either be inferred from direct measurements (from ground or space) or reconstructed based on proxy data such as total ozone and sunshine duration". While satellites cannot make any direct measurements on the surface of the Earth, you could revise the text e.g. by saying "Physically, UV trend can only be detected from direct measurements on Earth. Reconstructed data can be based on proxy data such as the abundance of ozone, solar irradiance, sunshine duration, or regional reflectivity of the earth-atmosphere system measured from the space."
- On p.5: "Clouds induce more variability in surface UV irradiance than any other geophysical factor" is perhaps missing the words "…besides the solar elevation".
- Ch. 4.1. is utilizing the results given in Ch.4.2. Should the order of presenting the results be changed, i.e. the trends first and then the change point analysis?
- P.19, lines 22-23 say "...the stations with comparable latitude to Uccle (45–55N, stations in blue in Table 6), the trends in UV range from −2.1 to +8.6% per decade". Two copmments: Firstly, the downloaded pdf copy does not show anything in blue, and secondly, Hoher Sonnblick at 47.05N suggests a trend of 14.2%/decade.

The list of references is impressive but you may want to add the following two:

- P.4 line 17: Lindfors et al, 2007: A method for reconstruction of past UV radiation based on radiative transfer modeling: applied to four stations in northern Europe. J. Geophys. Res., Vol. 112, D23201.
- P.4 line 23 and in Ch. 4.3.1.: Eleftheratos et al, 2014: Ozone and Spectroradiometric UV Changes in the Past 20 Years over High Latitudes, Atmosphere-Ocean, DOI: 10.1080/07055900.2014.919897

The figures and tables are clear and the following two comments are given:

- Fig.2 and 3: the unit of the y-axis is missing.
- Fig.4, 8, and 9: the axis labels and the scale could be larger for a more easy reading.
