Response to Anonymous Referee #1

Our responses follow the reviewer's comments (in bold). Since page and line numbers of the original manuscript are different in the new version, the new page and line numbers (in the version with marked changes) are also given where needed.

General comments

The manuscript by Fountoulakis et al. analyzes changes in solar spectral UV irradiance at 307.5, 324, and 350 nm at Thessaloniki from measurements of two Brewer spectrophotometers. The observed changes are then interpreted in terms of changes in aerosols and total ozone column at this site. The paper is well written, provides a comprehensive overview of the context and methodologies, is well referenced (in particular the introduction), and presents new data from this important site. I recommend publishing the manuscript in ACP, provided that my specific comments below are addressed.

Specific comments

P35756, L6: I believe that Zerefos et al., (2012) did not conclude that the observed "slowdown or even a turning point in the upward UV-B trends after 2006" was "mainly due to a corresponding turning point in the negative trends of aerosols." For example, Figure 1 of Zerefos et al., (2012) does not indicate that the negative trend in AOD has slowed down after 2006. The lowest value is in 2011 at the end of the time series. Please double-check this assertion and modify, if appropriate.

Response

(P3, L6-12) The comment is correct. The manuscript has been revised accordingly.

P35758, L13: A 1-sigma uncertainty of 5% seems high. Please double-check that this is indeed 1-sigma.

Response

(P5, L10-14) The 1σ uncertainty of 5% has been estimated in Garane et al. (2006) and refers to the overall uncertainty due to all possible sources in the measurement and calibration processes. The text has been revised for clarity.

P35759, L27: 60 Hz? Was the temporal resolution really this high, i.e., 60 samples per second? Or was it 1/60 Hz, i.e. one sample every minute?

Response

(P6, L22) This was a mistake. The actual frequency is 1/60 Hz (one measurement per minute). The text has been revised.

P35762, L10: As far as I understand, the "magnitude of trends" (i.e., the regression slope) is not affected by autocorrelation, only the significance of the trend is. Please double-check.

(P8, L29) The reviewer is right; thank you for pointing this. The discussion in this section has been revised.

P35762, L28: I assume the daily anomalies were calculated by subtracting from each data point the average of all data points from different years but with the same day-of year as the data point in question. Perhaps this could be clarified by adding "for that day" after "climatological value". In the following line, change: "Then using the daily anomalies we derived monthly mean anomalies" to "Monthly mean anomalies were calculated by averaging the daily anomalies for months with at least 10 . . ."

(P9, L15) We revised the text according to the reviewer's suggestions.

P35763, L15: Was the assertion that "yearly mean anomalies are not autocorrelated" confirmed with a statistical test or is this an assumption?

(P10, L1) It was an assumption which was not adding any useful information to the manuscript, thus it has been removed. In fact, this statement could be even wrong under certain circumstances, when, for example, the yearly mean irradiance is monotonically increased due to long-term negative trend in aerosols.

P35764, L12-17: The description of trends that are significant and trends that are not is rather convolved. Please simplify the language! Perhaps enumerate the conditions for which trends are significant and mention that for all other conditions, trends are not significant.

(P11, L5-6) We made an effort to simplify this paragraph.

P35767, L22: According to this line, the turning point analysis was based on the analysis of _yearly_ mean anomalies. However, in section 3.1 (P35762, L18ff), it is stated that "the methodology of Yang et al. (2006) was applied on the _monthly_ mean anomalies." So was the analysis based on yearly or monthly data?

(P14, L5-7) The turning point analysis is based on monthly mean data as it is now clarified in the manuscript (P14, L10-12)

P35769, L10: Delete "extremely". (E(307.5) was lower in 1997 and 1998 and even in 2010, the anomaly was less than 5% - hardly "extreme".)

Response

Done

Figure 5: AOD anomalies have a large spike in 1999. Why is there no corresponding spike of opposite sign in E(350) and E(307.5)? Is there perhaps something wrong in the AOD dataset for that year?

Response

(P14, L5-7) In 1999, very high levels of AOD were measured by both Brewers operating at LAP. Since different methodologies were used to derive the AOD from the two instruments (Kazadzis et al., 2007), we consider these results independent and consistent. A possible explanation of this is disused now in (P15, L8-15).

Caption Figure 5: Mention that data shown in Figure 5 are based on clear-sky data. (Otherwise, the small decrease in E(350) after 2006 that occurs despite decreasing AODs could be explained by an increase in clouds).

Response

Done

Technical corrections

P35760, Eq. (1) and line 17: Please use either "a" or "alpha" in "alpha(lambda, theta_0)

Response

Done

P35763, L3: Remove dash in "multi-linear" to be consistent with later spelling of this word.

Response

Done

P35768, L17: Delete "all"

Response

Done

P35770, L8: Replace "ensure data of superior quality" with "increase the confidence in the accuracy of the spectral measurements".

Response

Done

P35770, L15: Replace "higher" with "larger"

Response

Done

Throughout the paper, including the references, replace "Hader" with "Häder"

Response

Done

Figures 3 and 4: Labels on x-axis in Figure 3 are upper case while they are lower case in Figure 4. Please harmonize. Also in Figure 4, replace "automn" with "autumn". In addition, it is a bit confusing that errors bars are 1 sigma while the caption discusses significance at the 95% level. Consider using 2 sigma error bars such that significance becomes visually better apparent.

Response

Labels in both figures are now lower case, and "autumn" in figure 4 has been corrected. The reviewer is right that 2-sigma error bars would give a better visual representation of the 95% significance. However if we apply this change in figure 4, for consistency it should be also applied in figures 2 and 3. In both cases the y-axis should then be expanded, which in turn would reduce the clarity of the presented results and would make more difficult for the reader to, e.g., distinguish the differences between the clear-sky and the all-sky trends. Thus, we kept the 1-sigma error bars in the figure and we moved the discussion for the statistical significance in the main text (P11, L27-29).