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Interactive comment on "Long-term series of surface solar radiation at Athens, Greece" *by* Stelios Kazadzis et al.

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This study presented all-sky SSR data observed in Athens from 1952 to 2012 and extended the data back to 1900 based on a statistical relationship with SD data. They also derived clear-sky SSR data by removing data on cloudy days from the all-sky SSR data. They documented the data sources in detail and inspected the trends and variabilities of the SSR data in separate time periods and with different time resolutions (daily, monthly, and annually). Among others, the paper particularly explored the question as to what are the sources of decadal variabilities in SSR data in Athens: aerosols or clouds. They compared the SSR data with cloud cover and AOD data and concluded that the SSR decadal variabilities were caused mostly by aerosols rather than clouds.

The significance of this paper may be limited because they deal with the SSR data only

at one location (Athens) and support/confirm previous results drawn from analyses dealing with wider regions (countries and continents) as evident in Table 4 (also, for example, dominant role of aerosols in determining the decadal SSR trend in Europe (Norris and Wild 2007; Ruckstuhl et al. 2008; Wang et al. 2012)). On the other hand, the long-term SSR data including those reconstructed and the accompanied analysis does make a contribution to the debate associated with global dimming and brightening (Wild 2009).

Furthermore, the paper discusses relevant previous studies in Sections 1 and 4, but it also needs to state more explicitly what are scientifically new in this study. In terms of the presentation, I think that the text could generally be condensed a lot by organizing the argument. I spotted a number of errors in writing and pointed out several of them as minor comments.

As a main point, I argue that, while the authors looked into the SSR data from several angles to arrive at the abovementioned conclusion, they failed to show a straightforward analysis looking directly into the correlation between the all-sky SSR and the AOD. As far as I understand, the evidences they provide in the current manuscript are essentially a combination of high correlations between the clear-sky SSR and the AOD and also between the clear-sky SSR and the all-sky SSR (Figure 11 and text). Also, as a counter evidence, they indicated a weak correlation between the all-sky SSR and the cloud octa (Figures 8 and 9). These evidences all point toward the dominant role of aerosols in determining the all-sky SSR, but these are all indirect evidences. I think they could make the same point more clearly by directly presenting the relationship between the all-sky SSR and the AOD.

I have minor comments as follows:

- The abstract states that a decrease of 2.9%/decade in SSR from 1910 to 1940. I wonder why the trend from this particular period is selectively highlighted within the extrapolated period of 1900-1952. Table 2 indicates a small increase of 0.04%/decade

from 1900 to 1952, which is clearly different from above.

- Also in the abstract, I could not find where the difference of 4.5% comes from. Table 2 indicated approximately 3.1% but for a slightly different period. The winter period shows the largest change, unlike what is stated in the abstract.

- Page 2, Line 21: Regarding the discussion on SSR changes in polluted and pristine areas, I believe that this is still an issue of controversy but two recent studies (Imamovic et al. 2016; Tanaka et al. 2016) showed otherwise, which can be reflected to this statement.

- Page 3, Line 2: Fix the citation style.

- Page 3, Line 6: Remove “explain”.

- Page 3, Line 22: Figure 1 of (Ohmura 2009) also makes a clear case for this statement.

- Page 4: Somewhere in the text (not necessarily in this page), the discussion could touch on aerosol-clouds interactions to acknowledge that the two factors (aerosols and clouds) are not completely mutually exclusive in explaining SSR trends.

- Page 11, Line 14: I am trying to speculate what causes the weak correlation in summer. The paper cites small ranges of variables in summer as a reason for weak correlation, but how exactly do the range affect R2 values? Later in the paper (Figure 12), the number of cloudless days in summer is generally large, compared in other seasons. Could the number of cloudless days influence the correlation level?

- Page 12, Line 10: “light grey” should be “light blue” from what I can see from the figure.

- Page 13, Line 1: Separate “late1930’s” into two words.

- Page 13, Line 7: It may be useful to break up the 1900-1952 period into two because the text discusses the trend till late 1930s and the trend that follows separately.

- Page 13, Line 11: Remove comma after 2012.

- Page 14, Lines 10-13: It needs to be specific which region it refers to. The trend of global anthropogenic BC emissions during 1910-1950 does not decline but rather levels off (Lamarque et al. 2010).

- Page 15, Line 7: Remove comma after Figure 6.

- Page 16, Line 3: Is the left panel of Figure 7 essentially same with Figure 5? If so, the left panel does not have to be shown as it is redundant.

- Page 18, Line 9: “non significant” needs to be connected by hyphen.

- Page 20, Line 11: the SSR line should be “black” rather than “blue”.

- Page 22, Line 7: Would there be any possible explanation why only the clear-sky SSR trend in winter is negative? A similar result was obtained for the all-sky SSR (Table 2).

- Page 22, Line 25: The discussion on visibility can be part of the discussion, not the conclusion. Visibility has not been brought up since the introduction.

- Page 23, Line 3: “drown” should be “drawn”.

References

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