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

Anonymous Referee #3

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The paper analyzes a long-term series of surface solar irradiance observations made at Athens between 1953 and 2012. Estimates of the surface solar irradiance for the period 1900-1952 are obtained by applying a simple relationship between sunshine duration and irradiance, fit to the available data.

The study partly confirms previous findings on the long-term evolution of surface solar radiation in Europe. However, little information is given on the coherence of long-term calibrations, homogeneity of applied corrections, measurement uncertainties, absence of drifts and step-changes. These aspects should be thoroughly analyzed. In my opinion, the understanding of these aspects is necessary when analyzing such a long dataset: the reader should be convinced that the dataset is suitable for this type of analysis, and what are the estimated uncertainties and limitations.

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Discussion paper

Moreover, the authors do not address the possible role of local urban emissions and possible associated changes of atmospheric transmissivity, except for a short comment in the conclusions. Atmospheric water vapour may have played a role as well. Athens has changed dramatically during the investigation period (see e.g., Founda et al., 2016), and the effects of local emissions might emerge.

In my opinion, these aspects need to be analyzed and discussed; in particluar the assessment of the dataset reliability and a deeper discussion of the involved uncertainties are necessary. I suggest that the paper is re-submitted after including these aspects and verifying the robustness of the dataset.

Specific points follow.

Page 3, line 4: AOD trend of 0.05 per decade: in what period?

P. 3, l. 9: "This attenuation may be much larger ..."

P. 5, l. 17: "Maximum error on the daily integral SSR..."

Table 1 and associated discussion: there are relatively long periods between instrumental changes (up tp 6 years). How the radiometers were calibrated prior to 1992? Which was the reference scale? Were the instruments compared with the old one before substitutions? Was the occurrence of instrumental drifts checked?

P. 6, l. 6-12: the application of different data selection criteria, with the addition of quality checks based on the diffuse irradiance, may potentially influence the results of the trend analysis, Did the author check that this is not the case?

P. 7. l. 1-2: as far as I understand, the night-time dark signal was subtracted from daytime measurements. This procedure reduces but does not elimnate the thermal offset of the instruments. It must be taken into account that the different types of radiometers display a quite different thermal offset; in general, this is much larger for PSP than for 8-48 or 180° pyranometer. Thus, a systematic overestimate of the SSR in daytime, up to 3-4 W/m2, is possibly present in the data after 1989. This may

potentially produce an artifical positive trend in SSR in the recent year; at least, an additional uncertainty should be considered in the trend analysis. Did the authors take into account this effect?

P. 9, l. 31: how are SSR max and SDmax calculated?

P. 12, l. 4: SSRmi

P. 12, figure 4: estimated SSR/SSRmax values (figures 2 and 3) show a typical 10-20% spread around the fitting line. This is expected, since the used relationship takes into account only cloud duration. All other effects (most of the aerosol direct effects, as well as most changes in cloud properties) can not be reproduced by the method. What is the uncertainty associated with these SSR estimates? Was this uncertainty considered in the trend analysis? Can these data be reliably used for trend analysis?

Moreover: there are some rapid changes in the series that may require additional scrutiny; some of these seem to be in correspondence or close to the dates of the radiometers' replacements (e.g., possibly in 1960, 1968, 1973). This seems even more evident in figure 8 from the de-seasonalized monthly mean SSR. Was the presence of step-changes in the series, mainly in corrispondence with instrument replacement, checked?

Also, it is surprising that no significant signals of large volcanic eruptions (Agung in 1963, El Chichon in 1984, Pinatubo in 1991) are present in figure 4. A small SSR reduction in the early 90's, possibly related with Pinatubo, appears in figure 8; however, the minimum during 1990's in fig. 4 seems too late to be ascribed to Pinatubo (whose effect lasted for up to 2 years). Is there a possibe explanation?

P.13, l.2: the graph also shows a clear decrease during 1950's.

P.13, l.3: shows

P.13, l.4-5: see comments to fig. 4 regarding the Pinatubo effects.

P. 13, table 2: why were these periods chosen?

P. 13, l. 19-22: does the trend determination and its statistical significance take into account uncertainties?

P. 14, figure 5: this figure does not seem to support the choice of the periods used in table 2 for the trend calculations.

P. 14, l. 18-19: also here, it does not seem to me that the Pinatubo effect is evident.

P. 17, fig. 8: the units for yearly mean SSR and total cloud cover are missing in the graph. The evolution of the yearly mean SSR does not seem to be coherent with the annual series of de-seasonalized SSR in figure 4 (the minimum in early 1990's does not seem to coincide with the minimum in mid 1990's in figure 4; the minimum in 1970 in figure 4 appears earlier in figure 8). Is there an explanation for that?

P. 18, l. 7: "presence of" may be removed

P. 18, l. 8: figure 12 and the related discussion suggest that there is a long-term change in the number of cloudy days. Conversely, no significant change in the annual mean cloud cover appears. May this be taken as an indication of changes in cloud properties or distribution?

P. 18, l. 20: it may be emphasized that the clear sky selection criterion eliminates cases with high aerosol optical depth.

P. 21, l. 4-5: apparently, there is no stratospheric aerosol contribution in the ChArMEx AOD dataset. The large volcanic explosions are important events with an expected impact on SSR, and datasets which include these cases should be used. Please, explain more clearly what is the meaning of ".. uses the trend and not the interannual variability which is not included in the global model that was used".

P. 21, l.12-13: a change of almost a factor of 2 in the frequency of cloudless days seems to be non marginal. No evident effect appears on SSR in figure 8. However,

trends in table 3 are calculated in periods separated around the years with minimum number of cloudless days. May part of the trend change in the two periods due to the long-term change of cloudless days/cloud properties (see also comment to p. 18, l.8)?

P. 22, l.25-P.23, l. 9: this discussion seems not fully consistent with the conclusions of the paper. For instance, Founda et al (2016) show that visibility is strongly related with AOD; and the paper highlights a possible role of aerosols in affecting SSR.

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