

Response to the comments by Anonymous Referee #1

Received and published: 7 November 2015

ACPD 15, C9047–C9048, 2015 (A. Imamovic et al., Global dimming and urbanization)

Dear referee #1,

Thank you for your comments. In the following we divide your comment into seven parts and respond to each of them. The original comments are **grey and bold**. Our response is normal typeset. Excerpts from the manuscript are *italic*.

The full comment by the referee was:

This study addressed the issue whether urbanization has an important impact on the observed dimming rate of surface incident solar radiation. There are a number of studies on this issue and different conclusions have been derived. The authors designed complicated methods to do their job and their conclusions are similar to recent publications. I have several comments to help the author in presenting their results more clearly: (1) How the population index (PI) and its increase relate to the urban-rural contrast of their impact on the observed global dimming? It is unclear in the current manuscript. What the increasing and decreasing of PI should impact the urban-rural contrast of dimming? and what was the observed values? (2) The observations of China have shown to be impacted by the quality and their performance of the instruments used (Wang et al., 2015). How the authors addressed this issue? It should be discussed at least. Wang, K., Q. Ma, Z. Li, and J. Wang (2015), Decadal variability of surface incident solar radiation over China: Observations, satellite retrievals, and reanalyses, *J. Geophys. Res. Atmos.*, 120, 6500–6514, doi:10.1002/2015JD023420. (3) For the significant correlation of PI and dimming rate in China. During the study period, China population increased significantly and surface solar radiation decreased at same time. It is easy to understand the significant positive correlation between PI and dimming rate. However, the physical background is lacking here. PI can has a significant positive correlation with any data that show significant decreasing trend during the period. How to avoid the spurious correlation that they just occurred concurrent? (4) The author divided their result areas into Europe and Japan, Asia (China and Russia). It may introduce misunderstandings. It looks like the Japan is not in Asia, and Russia is in Asia not in Europe.

1)

The authors designed complicated methods to do their job and their conclusions are similar to recent publications

We are using 0.08° resolution population density data and devise a population index PI. The PI of a site is the sum over all population density grid cell of the dataset within a certain radius of the site. We introduced an inverse distance-weighting that gives greater weight to grid cells closer to a site. We do not consider the use of a PI as complicated. We performed sensitivity tests and showed that the results are robust against the choice of parameters (radius around the site, distance weighting) for the PI.

Our results are similar to the results found for Israel (Stanhill and Cohen, 2009) and for Japan (Stanhill and Cohen, 2008). Moreover, given the fact that we do not find any correlation between the temporal change of the population pattern surrounding a site and the SSR trend measured at sites for the sites in Europe and Japan, we are in line with the findings by Wang et al. (2014). However, the conclusions drawn from our study are different from the study by Alpert and Kishcha (2008), who argued that an

urbanization effect exists for the GEBA sites.

2)

How the population index (PI) and its increase relate to the urban-rural contrast of their impact on the observed global dimming? It is unclear in the current manuscript. What the increasing and decreasing of PI should impact the urban-rural contrast of dimming? and what was the observed values?

The issue with the urban-rural contrast and the potential for spurious biases when urban and rural sites from different regions are compared is thoroughly discussed by Wang et al. (2014). We refer to this important finding in the present manuscript (MS hereafter). Our study avoids aggregating sites into urban and rural ones, as this requires a (somewhat arbitrary) PI-threshold. The urban-rural contrast needs to be certainly considered, if sites are aggregated into groups and average SSR trends are calculated, which we do not do in the present study.

The estimated SSR trends can be seen in e.g. Fig. 3 in the MS.

3)

The observations of China have shown to be impacted by the quality and their performance of the instruments used (Wang et al., 2015). How the authors addressed this issue? It should be discussed at least. Wang, K., Q. Ma, Z. Li, and J. Wang (2015), Decadal variability of surface incident solar radiation over China: Observations, satellite retrievals, and reanalyses, *J. Geophys. Res. Atmos.*, 120, 6500–6514, doi:10.1002/2015JD023420.

We are fully aware of potential data heterogeneities in the used GEBA dataset. GEBA sites data quality is discussed by Gilgen and Ohmura (1999). We refer to this in the MS. We are using only GEBA sites that received the highest quality flags. We were not aware of the abovementioned study for China. We will discuss and incorporate it in the discussion in our revised MS.

4)

For the significant correlation of PI and dimming rate in China. During the study period, China population increased significantly and surface solar radiation decreased at same time. It is easy to understand the significant positive correlation between PI and dimming rate. However, the physical background is lacking here.

We presume you mean negative (instead of positive) correlation in the third and fourth line in the above excerpt from your original comment. Note that we are not comparing correlations between single SSR time series and PI time series for every site – we are estimating SSR trends and comparing these against the respective PI change of regional groups of sites. This does not necessarily imply a significant correlation.

The physical background is not discussed in this study, because this study is not aiming at answering the question what caused the global dimming. We are focusing on relationships between the change in population pattern around a site (expressed in terms of a population index) and the SSR trends to see whether SSR trends go hand in hand with PI increases of the site.

5)

PI can has a significant positive correlation with any data that show significant decreasing trend during the period.

Our study shows that the first statement in the above comment is not correct: e.g. the sites in Japan experienced large average increase of the (n=1) PI from 1960-1990. The average increase was even larger than the average increase of the sites in the "Asia" group as can be seen in Fig. 2. Nonetheless, no negative correlation between the change in PI and the SSR trends (Fig. 3) was found.

6)

How to avoid the spurious correlation that they just occurred concurrent?

We acknowledge that you are raising an important point. This issue applies not just to our study but obviously to all the population-based approaches. In light of the limitation of what the population data might have indicated, the conclusions derived from our analysis are kept conservative in the current MS. It is important to note, that we are not claiming that there is an urbanization effect for China. However we can't preclude it when PIs are used. This is stressed in the present MS:

[Line 6 on page 31143] *However, an urbanization effect for the sites in China and, particularly, for those in Russia cannot be ruled out. Further research is required to clarify the importance of urbanization for SSR trends during the dimming period.*

In the light of the data heterogeneity study for the sites in China in particular (Wang et al., 2015) and the potential for spurious correlation (Wang et al., 2014) an urbanization impact (inferred from population density China) for the global dimming in China must be interpreted very carefully. We will consider a more detailed discussion in the MS.

7)

The author divided their result areas into Europe and Japan, Asia (China and Russia). It may introduce misunderstandings. It looks like the Japan is not in Asia, and Russia is in Asia not in Europe.

The "Asia" group in our study consists of 39 Chinese, 22 Russian and 4 remaining, i.e. non-Chinese and non-Russian sites. Russia is a transcontinental country and belongs to both Europe and Asia. Since most of the Russian sites used in the present study are in the East we added the Russian sites to the "Asia" group. Note that the "Asia" group was split later for a separate analysis. Data abundance in Japan (45 sites) was comparably large. Hence we aggregated it into a separate group.

Although the site distribution map in Fig. 1 in the manuscript should resolve potential misunderstandings, we will stress in the MS that by "Asia" we mean mostly Russian and Chinese sites as well as four other sites in the Asian region, excluding Japan.