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Interactive comment on "Is global dimming and brightening in Japan limited to urban areas?" *by* Katsumasa Tanaka et al.

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[Referee's comment] The paper describes that the decadal trend of the surface solar radiation (global dimming and brightening) in Japan is a phenomenon by local air pollution or the large scale changes in background aerosols. This is an important point to investigate a cause of the global dimming and brightening. The authors separate the observatories into the polluted and pristine stations by carefully checking the historical land use map, population time series, satellite image, and actual site visits. This approach is reliable and would be useful in studying the cases in the other countries. The paper is generally well written and fits the scope of ACP. I think the paper could be published after minor changes.

[Our response] Thank you for reviewing our paper and providing us with detailed com-

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ments. All the comments were helpful to refine our manuscript. Please see our point-by-point responses to your comments below.

Technical comments: [Referee's comment] 1. Check all the in-text citations. For example, "(Ohmura and Lang, 1989)" in P2, L3 should be written by "Ohmura and Lang (1989)".

[Our response] We have converted all the in-text citations to the proper form.

[Referee's comment] 2. Figs. 3, 4, 5, 6, and 7. The scale bars are not clear.

[Our response] We generated these figures by using Google Earth Pro. The size of scale bars was already determined to what we thought optimal. As far as we are aware, there is little room to do further within Google Earth Pro. To improve the scale bar visibility in the revised manuscript, we thus worked directly on the images by adjusting their contrast and sharpness. The end results are somewhat better. Furthermore, if this paper is published in Atmospheric Chemistry and Physics, the journal might be able to offer larger printing space than that in Atmospheric Chemistry and Physics Discussions, in which these figures could be shown larger, which would help make the scale bars more visible.

[Referee's comment] 3. Figs. 5, 6, and 7. The legends indicating the urbanization (e.g., residential area) are necessary.

[Our response] We are afraid that we do not understand what exactly this particular comment suggested. It may be unclear what the red and green rows mean when these figures are inspected alone. In the caption of Figure 5, we thus added the following sentence: "Rows for the eight polluted and six pristine observatories are indicated in red and green, respectively." We did not add this sentence to Figures 6 and 7 because the captions for Figures 6 and 7 refer to that for Figure 5.

[Referee's comment] 4. P15, L21-28. The references of Ramanathan 2007a and 2007b are same.

[Our response] Thank you for pointing this out. We did not notice this duplicate. These two references are merged in the revised manuscript.

General comments: [Referee's comment] 1. P3, L24-30. The approach using both the population data and the land use map is a reliable method. However, if the population data is enough to show the urbanization, it is useful to classify the observatories worldwide into the polluted and pristine stations. The change in the population is strongly related to the land use. The population does not increase without the increases of the residential area and the commercial facilities. Is the land use map necessary in the classification?

[Our response] This comment is related to one of the very motivations for this study. It is indeed a common practice to use population data as a proxy for urbanization. However, in the debate on the possible causes of global dimming and brightening phenomenon, this approach is questioned. Our manuscript contains the following paragraph in the method section, which we hope clarities your concern: "Population data are the most accessible and only available long time-series on such a fine scale (note that municipality population data before 1970s are not systematically available in a digitalized format). There are, however, known limitations when they are interpreted as proxies for urbanization. The use of population alone as a proxy to infer the air pollution level is under debate (Alpert et al., 2005; Ramanathan et al., 2007; Alpert and Kishcha, 2008; Stanhill and Cohen, 2009; Wang et al., 2014; Imamovic et al., 2016). Related are issues associated with the use of population-based proxies to infer CO2 emissions (for an overview, see Andres et al. (2012)), even though CO2 emissions are not necessarily correlated with SO2 emissions on the relevant spatial and time scales (Grossman and Krueger, 1995; Holtz-Eakin and Selden, 1995; Dinda, 2004). Population data may serve as a proxy for CO2 emissions from residential and commercial sectors but work poorly for emissions from power and transport sectors. With respect to power sectors, population fails to represent CO2 emissions when coal is combusted in a remote area to support the electricity demands in a distant urban area. This type of issues

becomes important when one deals with emission data on a fine spatial scale. Concerning transport sectors, transport emissions per capita are known to decline above a certain population density threshold (Gately et al., 2015). In our analysis, the population time-series are therefore complemented by the series of historical land use maps, recent satellite images, and current photographs, which provide additional insights into how the site surroundings have been changed during the past period of interest."

To emphasize this argument upfront in this paper, we revised the following sentence in line 24 on page 3 (the underlined part was added in the revised manuscript): "While a few earlier studies (e.g. Alpert et al., 2005; Alpert and Kishcha, 2008) rely primarily on population data to infer the influence of urbanization on SSR measurements, we collect and utilize as much information as possible to overcome possible limitations for population-based proxies when they are applied to infer the air pollution level (Ramanathan et al., 2007; Stanhill and Cohen, 2009; Wang et al., 2014; Imamovic et al., 2016)."

[Referee's comment] 2. P11, L19-24. The trends of the transmittance are in line with those in the surface solar radiation. I would like to know the change in the transmittance can explain the changes in the surface solar radiation quantitatively. Could you estimate the change in the surface solar radiation from the change in the transmittance? I think that the direct component of the surface solar radiation can be estimated roughly but it is difficult to estimate the diffuse component.

[Our response] Such computations would require a radiative transfer model like MODTRAN. However, using such a model would lead to another new study. For this paper, we would thus stop here without going into such numerical computations.

We appreciate this suggestion from a general point of view. We in fact did not analyze the data in a quantitatively rigorous way. We rather focused on the collection of data (in terms of both the quantity and quality), which itself is a project, and we interpreted them mostly by visual inspection. Some of the collected data (e.g. maps) cannot be

even quantified. Nevertheless, the data we collected offer a new opportunity for more quantitative analyses, which is a future research direction.

[Referee's comment] The annual means of the transmittance are calculated from the data under the clear sky condition. Such data may not be a representative of aerosols in a year. Please show how many days of the transmittance data are used to calculate the annual means.

[Our response] When there are more than six months of transmission data available within a year, we take the mean and plot it as an annual-mean value. The threshold of six months is arbitrary. Another way would have been to fill the missing 6 months with the climatological mean months that can be determined from the same site. This would exclude the possibility that biases can be introduced in the annual means by not sampling a part of the year (which for example could be on average higher or lower than the 6 months available). However, we don't think it feasible to implement this at this stage of the review process.

The original manuscript was not explicit about how we arrived at the annual-mean transmission data. We thus add the following sentence to the revised manuscript in line 15 on page 10: "When there are more than six months of transmission data available within a year, we take the mean and use it as an annual-mean value."

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