

Dear Dr. Katsumasa Tanaka,

Thank you so much for taking time and energy in reviewing and improving this paper. We are delighted to receive your insightful suggestions. We also thank you for the positive comments on this study.

Your main concern is about the statement on the effective air pollution regulations in China after 1995. Regarding this, you raised two major issues. The first is about the specific aspects of the laws and regulations mentioned in Figure 6. With your question, we will double check the detailed information on all the environmental laws and regulations. Only those having possible effects on air quality protection will be picked out in the revised version. Figure 6B will then be redrawn and the corresponding statement in both caption and text will be modified.

The second major issue is about the air pollution trend in China. We understand your concern. Actually, the globally well-known hazy pollution problem in China is not a nationwide phenomenon. This is indicated in Fig. 2 in that sunshine dimming mainly continues in the area of the North China Plain, where haze pollution is being reported, while brightening is prevalent in South China after 1990. Besides, the severe hazy pollution problem in the North China Plain only occurs in the winter season. The increasing emission trend mentioned in the references of Ohara et al. (2007), Lu et al. (2011) and Lin et al. (2014) is in accordance with the increasing urbanization level in China (Fig. 5), and thus consistent with our main conclusion. In the dimming phase without effective pollution regulations, the emissions generated during the urbanization process were directly changed into equivalent pollutants. By contrast, in the brightening phase, the increasing emissions were compensated by the clean air policies and investments. Therefore, urbanization no longer simply means an increase in air pollution in the brightening phase, and its effect on sunshine duration trends becomes insignificant. But we agree with you that the reason for the recent leveling off in sunshine duration trend in China is still an open question and needs further discussion. We will add a similar discussion in Section 3.2 of the revised manuscript.

In the following context, please find the point-by-point responses to your specific comments.

Comment 1: Page 1, Lines 15-16 From the abstract alone, it is not clear what the numbers (86% and 84%) indicate. It is also unclear what “a large overlap” means.

Reply 1: Thanks for this comment. In the revised manuscript, Lines 15-16 of the abstract will be modified into “Urban and rural sunshine duration trends show similar spatial patterns for the global dimming and brightening phases, respectively.”

Comment 2: Page 1, Lines 27-28 Because of the reasoning in the overall comment above, I do not think that this claim is substantiated.

Reply 2: Please refer to the replies to your major comment#2 shown above.

Comment 3: Pages 2 and 3 In spite of the conflict of interest, I cannot ignore my recent work (Tanaka et al. 2016), which deals with the very questions addressed in this study. Please consider incorporating (Tanaka et al. 2016) in the discussion if you agree to do so.

Reply 3: The reviewer is right. We should cite this recent research on urbanization effect. Thank you so much for sharing it with us! We will add this reference at the end of the 1st paragraph of the Introduction section as “A very recent work of Tanaka et al. (2016) put forward a new approach to infer urbanization by combining population data, historical land use maps, satellite images and site visit experiences, and further proved that the global dimming and brightening phenomenon in Japan was not restricted to urban areas and not primarily driven by local air pollution.”

Tanaka, K., Ohmura, A., Folini, D., Wild, M., and Ohkawara, N.: Is global dimming and brightening in Japan limited to urban areas?, *Atmos. Chem. Phys. Discuss.*, 2016, 1-50, 10.5194/acp-2016-559, 2016.

Comment 4: Page 2, Line 11 As far as I can see, (Liley 2009) does not assert the fact that the slope-related problem found in (Alpert et al. 2005) persists in (Alpert and Kishcha 2008). (Liley 2009) raises a different problem for (Alpert and Kishcha 2008).

Reply 4: Sorry for this mistake. We will correct this statement by “Liley (2009) disputed the conclusions made by Alpert and Kishcha (2008) and claimed that a large anthropogenic effect in the vicinity of dense population does not negate the finding of long-term change in sparsely populated regions, where a downward trend of $-0.16 \text{ W m}^{-2} \text{ year}^{-1}$ was given in their previous work of Alpert et al. (2005).”

Comment 5: Page 3, Lines 1-4 The discussion here is a little too short, I think. It would be helpful if the authors discuss some more issues when one compares sunshine duration records with surface solar radiation measurements. As the authors wrote, sunshine duration records have a wider spatial and temporal resolution, which is a clear advantage, given the lack of surface radiation data. But are they almost always consistent with surface solar radiation records?

Reply 5: Great suggestion! According to your suggestion, we will reinforce this discussion as follows:

“Furthermore, sunshine duration is used as the proxy for surface solar radiation. Compared with solar radiation, sunshine duration has a much wider spatial and temporal coverage and is almost free of inhomogeneity in China (Xia, 2010; Wang and Yang, 2014; Wang et al., 2015). For the whole China (latitudes $16^{\circ}32' - 52^{\circ}58' \text{ N}$ and longitudes $75^{\circ}14' - 132^{\circ}58' \text{ E}$), there are 130 solar radiation stations, with only 59 stations covering at least 85% of the measurement period from 1958 to the present. Moreover, the majority of solar radiation stations are located in urban areas, which will further limit the available data samples for studying urbanization effects. In contrast, there are 906 sunshine duration stations, thus exceeding the solar radiation stations by a factor of about 7. Besides, 638 of them have sunshine duration records which cover at least 85% of the whole measurement period since the 1950s. It has been demonstrated that sunshine

duration is able to capture variations in cloudiness and signals from aerosol concentrations (Sanchez-Lorenzo et al., 2009; Wang et al., 2012c; Sanchez-Romero et al., 2014, 2016; Li et al., 2016; Wild, 2016). Similar with the solar radiation trend, a transition from decreasing to levelling off was also noted in sunshine duration trend in China around 1990 (Wang et al., 2013, 2014). Nevertheless, the existence of an urbanization impact on the trend of sunshine duration remains unclear.”

Comment 6: Page 3, Lines 6-8 From reading it, it does not come clear to me why these questions are worthy of exploration. I suggest that the authors elaborate a bit more to convince readers of the importance of such questions. Also missing are some clear statements on what are actually new in this paper. There are closely related studies like (e.g. (Wang et al. 2012a; Wang et al. 2012b; Wang et al. 2013; Wang et al. 2014)) as cited in this paper. Some specific statements on what are different from previous studies would clarify the value of this study. This is a question of writing style, but it is usually more common to write “three questions” than “3 questions.”

Reply 6: We agree with you. Based on your comments, we will modify the last paragraph of the Introduction section as follows:

“This study will then make a first attempt to examine the urbanization effect on sunshine duration variations in China. The wide temporal and spatial coverage of sunshine duration observations in China provides a unique opportunity to fully understand the differences of the dimming and brightening phenomenon between rural and urban areas. The value of urbanization as an indicator of pollution level will be evaluated for different stages of social and economic developments in China. In conclusion, the effect of urbanization on sunshine duration variations will be quantified.”

Comment 7: Page 3, Lines 14-15 This sentence structure needs to be fixed.

Reply 7: We will modify this sentence into “During the period studied here, instrument replacements only occurred in < 2% of all the meteorological stations (Tao et al., 1997; Wang et al., 2015)”.

Comment 8: Page 4, Line 7 A few more digits should be shown for 0° N and 0° E? Otherwise, these are not very useful.

Reply 8: We will modify this sentence into “Finally, 172 urban-rural station pairs all over China were chosen with an average difference of 95 m in elevation and negligible differences in average latitude (~0.001°N) and longitude (~0.002°E)”.

Comment 9: Page 4, Lines 7-8 Why are the 19 stations replaced?

Reply 9: Sorry for the confusing statement. We will correct the sentence as “Records from 19 of the selected stations (~5.5% of total) were completed based upon their collocated stations with similar climatic and administrative conditions.”

Comment 10: Page 4, Line 23 “100%” on the right hand of equation (2) probably mistakenly entered into the equation.

Reply 10: You are right. It will be deleted in the revised version.

Comment 11: Page 4, Line 24 Please fix the unit for land area.

Reply 11: Thank you so much for noticing this mistake. We will correct it by “land area (km²)”.

Comment 12: Page 5, Section 3.1 Previous studies need to be integrated in the discussion here because this conclusion has been shown by several others.

Reply 12: Yes, you are right. We will add a new sentence (after line 13, Page 5) with more previous works to discuss the conclusions drawn in Section 3.1 as follows:

“Although the dimming rate at the rural sites is lower than at the urban sites, this yet provides no convincing evidence that sunshine dimming is exclusively in urban area and hence a local phenomenon. Dimming in rural/sparsely populated areas was also noted in Europe, Japan, New Zealand, Egypt and the Greater Tel Aviv region in previous studies (Liley, 2009; Robaa, 2009; Stanhill and Cohen, 2009; Wang et al., 2014a; Tanaka et al., 2016). The averaged urban dimming rate in China is slightly lower than that obtained in previous studies (Wang et al., 2013; Wang and Yang, 2014), because a few mega cities without available nearby rural stations were excluded from this study.”

Comment 13: Page 5, Line 30 Does it sound better if “highly correlated” is replaced by “accompanied by”?

Reply 13: We will replace “highly correlated with” by “accompanied by” in the revised manuscript.

Comment 14: Page 7, Line 19 The data suggest “decrease” rather than “saturate.”

Reply 14: The reviewer is right. The word “saturate” will be replaced by “decrease” in the revised manuscript.