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Interactive comment on "Urbanization effect on sunshine duration during global dimming and brightening periods in China" *by* Yawen Wang et al.

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Overall comments

This paper investigates if and how air pollution might have affected the sunshine duration records in China over the past several decades. This study particularly looked into records at pairs of urban and rural stations that are geographically proximate each other. The authors identified factors that explain the decadal trends of sunshine duration records at different locations in China by using several proxies for urbanization including population density, a few related constructs, industrial GDP, and related investments.

I enjoyed reading this paper, and I think this will potentially be a good piece of work. As far as I follow the literature, most of the conclusions derived from this study have also been shown by several previous studies (e.g. (Wang et al. 2012a; Wang et al. 2012b; Wang et al. 2013; Wang et al. 2014)). This study strengthens such earlier conclusions based on a newly performed analysis from various angles, providing deeper insights into what might have caused the changes in sunshine duration and also surface solar radiation records in China over the past several decades.

As a major comment, I only take issue with their argument related to the possible effect of clean air policies on sunshine duration records. The authors claim that changes in sunshine duration records since 1995 can be partly explained by recent gradual penetration of pollution regulations. Their claim is based on the number of laws and regulations for "environmental protection" in China since 1978 (Fig 6B). The paper argues that the recent emergence of environmental policies in China may explain the plateau in the trend of sunshine duration records over the last two decades there.

For this argument, I raise two issues as follows: First, no specific information is given for such laws and regulations, except for the link to the website of the Ministry of Environmental Protection of the People's Republic of China. I wonder if they are all really related to air pollution. Could it be that they include those related to eutrophication, for example? A clarification is needed here. Second, in contrast to what the authors derive from Fig 6B, severe air pollution in major Chinese cities is a globally well-known "current" issue that has been worsened or at least persisting over the past few decades. Data for most pollutant and aerosol precursor emissions show no clear indication for their declines (e.g. (Ohara et al. 2007; Lu et al. 2011; Lin et al. 2014)). Given this, I am not entirely convinced by the claim that pollution control in China contributed to the recent flat trend of sunshine duration there. Then, what would explain such a plateau in the sunshine duration trend? To me, this is an open question. Factors are needed to offset the rising trend of cloud cover over the past two decades. But I would think it is not clean air policies since the emission data indicate otherwise.

Overall, I suggest a revision to be further considered for publication in Atmospheric Chemistry and Physics. I have more specific comments as detailed below. With respect to writing, I do follow most part of the paper, but I think some text editing including English editing would help enhance the clarity of the paper.

Specific comments

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.

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

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.

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).

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?

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."

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

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

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

Page 4, Line 23 "100%" on the right hand of equation (2) probably mistakenly entered into the equation.

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

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

Page 5, Line 30 Does it sound better if "highly correlated" is replaced by "accompanied by"?

Page 7, Line 19 The data suggest "decrease" rather than "saturate."

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