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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global dimming and urbanization: did stronger negative SSR trends collocate with regions of population growth?" *by* A. Imamovic et al.

Anonymous Referee #2

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This paper is a useful contribution to past discussion on the causes of Global Dimming (GD). Though the paper does not add to the physical understanding of suggested causes of that phenomenon, it does clarify the relation with increases in population or population density, or rather the lack of such relation for Europe and Japan.

The importance of this question was demonstrated (and perhaps exaggerated) by its treatment in the IPCC's AR4 report. There, a large and increasing number of studies of GD were downplayed as representing local effects of air pollution, purely on the basis of the paper of Alpert & Kishcha (2008) associating GD trends with population increase. That work also left the physical mechanism implicit and unquantified, so the

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current paper can be seen as closer analysis of the claimed relationship.

That GD does not correlate at all well with PI increase, or PI in 2000, across the whole dataset is unsurprising. Even if local air pollution in the vicinity of sensor sites was a predominant cause of GD, presumably through direct or indirect aerosol effects, the relation to population density would be highly uncertain and dependent on the industrial and economic profile and lifestyle of that population. It is easy to see that the negative correlation of PI with SSR trend (or positive correlation with GD) in China and Russian Asia could be the product of (especially) coal-fired industrialisation, even as clean-air initiatives or moves to high-technology industry and better public transport produced the opposite effect in other places.

The authors of the current paper are right to avoid such speculation, which should be addressed with studies relating SSR change directly to industrial activity, urban transport, and other sources of aerosol, and to actual measurements and models of aerosol concentrations. Such studies also need to go beyond the question of statistical correlations of trends to consider the actual amounts and optical or microphysical properties of aerosols in relation to observed SSR change.

The focus of this paper is to address the question raised by Alpert & Kishcha; whether population increase or density can be used to explain changes in SSR as a local anthropogenic effect. Imamovic et al. usefully demonstrate that PI, when calculated carefully and with adjustment of weighting to examine sensitivity, does not serve this purpose.

I have no difficulty recommending it for publication largely unchanged.

Minor corrections:

P 31138, line 3: "Rationales ..." (not "Rationals ...")

P 31138, line 8: It might be better to use a symbol other than "d" for the area weighting of population cells, as it invites confusion with the use of "d(s,y)" for site to population

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cell distance. Is the exponent "n" in 1/dˆn the same as the exponent in "1/d(s,y)ˆn"? If so, what is the reason? Is this not just the correction from population density rho(y,t) to actual population in a cell? In that case, there would be no exponent "n".

P 31150: Why is only one decimal place given in the correlation coefficients for the 22 Russian sites?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31133, 2015.

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