

Entitled: Solar radiation trend across China in recent decades: a revisit with quality-controlled data

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The comments come from this reviewer's perspective of over two decades of work in an interdisciplinary, environment-centred but systems modelling based area, where the dynamics and uncertainty of environmental processes are investigated. Therefore the comments are focused on the methodological side, less so on the physical and discursive side of the text.

- (i) The Authors consider two input-output (I-O) models with solar radiation levels as output, with the inputs including selected instrumental data series. Both models are of static regressive type, using instantaneous values of the input variables. The first model belongs to the Artificial Neural Network class (ANN), and is a highly non-linear black-box one. The second model is physics based, with the equations derived from approximate specific physical relationships. These two models have been fitted to the large collection of solar radiation series, and good model fits have been obtained. It is not surprising at all with the ANN model, as these models are characterised by such a great number of parameters, that they will fit anything. The hybrid model also produces a good model fit. It would be interesting to see how significant are the various estimated parameters (coefficients) of this model.
- (ii) The simulated solar radiation data (model-synthesised series) can be used to provide an "informed interpolation" of the instrumental solar radiation series, where there are missing or anomalous values, and can serve in any QA procedures for the data. This is a valuable contribution of this work.
- (iii) The models' outputs are then used to produce basic trend estimates, and these estimates are compared in a qualitative manner. I have serious reservations regarding the methodology used here, expressed in some of the comments below.
- (iv) One serious issue with both models' validation, and therefore validity, is that the Authors chose to use estimation procedures not providing any uncertainty information in their output. Any comparisons between the individual model results or with the data are therefore highly dubious. The only statistical test provided is a t-test based on correlation coefficient. No attempt is apparently made to even check that the test's assumptions are fulfilled. In brief, how can you tell whether two fairly noisy series are distinguishable with no uncertainty information? To illustrate this statement - there is no clear way of telling much

about data presented in Figure 2. No wonder there is little comment on this figure in the text.

Indeed, the easiest and transparent comparison would be to provide the model outputs with uncertainty bounds – this would be clear to the readers and provide an unambiguous result. I strongly recommend that versions of models are used which provide such information. There exist ANN approaches where uncertainty estimates are generated. Similarly, the Hybrid Model can be used in a carefully set up Monte Carlo experiment to generate output uncertainty bounds.

- (v) The treatment of trends in the manuscript appears to be another major problem, and it is secondary to the lack of uncertainty information. The Authors effectively draw straight lines along the data series (or fit parabolae), with no substantiation for choosing these models. Why should the solar radiation follow any straight line? Why is there no uncertainty around these lines? Any statistics textbook provides information of how to calculate the uncertainty of trend lines, especially in linear regression. And then, what about the trend significance? As another manifestation of the apparent issue, the annotations of Figures 3, 4 and 9 indicate a substantial confusion regarding statistical and time series nomenclature. These figures do not present “trends comparison”, but estimates of *trend slope* or *average rate of change* comparisons (trend is the estimated relationship and cannot be expressed as a single value), and very limited comparisons too, given the lack of uncertainty information. The dots are located at the values defined by pairs of estimates of trend slopes coming from data or simulated data. These estimates are in fact not deterministic, and direct comparisons as if they were deterministic are simply incorrect. Each of these dots will have an ellipse of uncertainty (and that’s if we only take the first two moments of the distribution) around it, and until we know how large it will be, we cannot draw any statistically meaningful conclusions from these plots.
- (vi) I would also like to return to the issue of arbitrary definition of the trend as a linear change. Such an assumption is highly limiting, as it excludes the changes of the nature that the Authors refer to themselves – between dimming and brightening or vice-versa. In this context I suggest looking at a brief and focused review of trend analysis methods can be found in for instance: Bianchi, Marco , Boyle, Martin and Hollingsworth, Deirdre(1999) 'A comparison of methods for trend estimation', *Applied Economics Letters*, **6**: 2, 103 — 109. One of the reviewed approaches, particularly well suited to the present data is described in Becker *et al.* (in *Atmospheric Environment*, **42** (35), 2008, where non-parametric non-linear trends are fitted to data, providing full uncertainty information, allowing for stringent evaluation of trend significance at any point in time.

I realise that there is a tendency in various specialist areas of scientific research to coin own terms, including “localised” meanings for general mathematical and other terms which are well and precisely defined, and have been for decades if not longer. While I may try to understand the roots for this autarchic tendency, there is not only no need for it, but indeed it can be seen as counter-productive, as it impairs the core of scientific communication, the free exchange of ideas, gradually turning specialist science areas into islands with their own languages. However, now is the time, when in the wake of “Climategate” clear and transparent communication is most needed, when climate scientists need to make themselves understood to each and every other scientist and to the general public in order to avoid misunderstandings and mis-communications.

- (vii) Given the fact that the first (model fitting) part of the manuscript is far more substantive than the trend analysis, I would suggest that the current title does not reflect the contents of the paper, and would suggest for instance “*Model based solar radiation data validation and interpolation*”
- (viii) Perhaps less importantly, no attempt has been apparently made to take advantage of the spatial distribution of the large number of available series. They will all be highly correlated, with some regional variations, possibly subject to industrial factors, closeness to the coast or other land features. An analysis of principal components might serve to reveal such relationships and help with data QA, but I agree that this might be beyond the declared scope of the paper.
- (ix) The Authors are not specifying exactly which version of the Hybrid Model of Yang et al (op.cit) they are using. This would be worth clarifying so that the readers could replicate the results.

I suggest that the Authors compare the methods and illustrations they used with methods and recommendations in such works as Draper and Smith’s *Applied Regression Analysis*. As for suggested references for trend analysis and generally time series statistics, I would suggest texts such as Shumway’s *Time Series Analysis and its Applications*.