

Interactive comment on “Solar radiation trend across China in recent decades: a revisit with quality-controlled data” by W.-J. Tang et al.

W.-J. Tang et al.

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We would like to thank the reviewer for the comments and suggestions, which contribute to improve the quality of our paper. We have replied to all comments and suggestions, and will consider revisions based on the discussions. Please, find a detailed point-by-point response to each comment.

1. 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
C7530

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 characterized 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.

Response: Thank you for this comment! The hybrid model was developed several years ago with the coefficients being calibrated using data in Japan. Although the systematic biases of the model can be reduced if we adjust these coefficients, such an adjustment does little affect the trend of the predicted solar radiation.

2. 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.

3. The models’ outputs are then used to produce basic trend estimates, and the estimates are compared in a qualitative manner. I have serious reservations regarding the methodology used here, expressed in some of the comments below.

4. 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.

Response: The two models are independent models. One model is the ANN-based (Artificial Neural Network) model, which uses observed input (routine meteorological data) and output (solar radiation) data between 1994 and 2006 to train the ANN-based model and then the trained model is applied to simulate the output data by using input data between 1979 and 2006 at each radiation station. The other model is a physical model, which is not calibrated with any solar radiation data presented in this study, and it is just used to simulate solar radiation just by providing inputs data at each meteorological station. The inputs of the two models are routine meteorological variables, measured by China Meteorological Administration, and we believe that the uncertainties in these input variables (pressure, air temperature, relative humidity, and sunshine duration) are small. A relevant debate about this issue has already been presented in the response to referee #1 (see AC C7525, point 2), in which the reviewer might have misunderstood our procedure. A major issue is the input of the aerosol (or turbidity) parameter, which is used to improve the climatology of the predicted solar radiation. Although its uncertainties will make biases in the estimated solar radiation, such biases are systematic errors and would not affect the trends.

5. 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

C7532

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.

6. 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, MarcoBoyle, 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.

Response: We do appreciate this comment! It is a miss that we did not clarify the confidence intervals of a trend. We will follow some typical methods to quantify the trend uncertainties in the revised version.

7. 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”

C7533

Response: We have discussed this issue among the co-authors, and we still prefer this title, as our goal is to analyze the solar radiation trend. We believe the magnitude of the trend had been exaggerated in previous studies, due to using data without quality-control.

8. 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.

Response: Thank you for your suggestion! It may be a good idea for our future work.

9. 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.

Response: We actually used the latest version, i.e., the model presented in Yang et al. (2006). We will specify the exact version of the hybrid model in the revised version.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18389, 2010.