

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

**Anonymous Referee #2**

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I would like to thank the Authors for taking time to address my concerns.

I shall refer to the Authors' responses by the points in my original review.

(1) The Authors provide a comment, which if I understand it correctly, is slightly worrying, namely that the model output is not very sensitive to the model parameter adjustments. Even at the qualitative level, such a statement is quite a damning assessment of any model. This may be due to various reasons, overparametrisation could be one of them, but this is beyond the scope of this discussion, as the Authors have successfully produced model outputs (but see my other suggestions).

Points (2)-(3) - do not require addressing

(4) This is one of the main issues I signalled, and the Authors are not providing any  
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satisfactory response. This was a question about uncertainty of the model estimates. I am slightly confused by their statement that the second (physical) model is not calibrated with any solar radiation data presented in this study. The un-calibrated model fit will be poorer than if the model was calibrated using these radiation data. I cannot see why this has not been done. Normally the uncertainty data would have been a by-product of such model fitting/calibration. Given the third party calibration, perhaps the Authors could generate a set of Monte Carlo simulations (model responses) given the estimated parameter uncertainties that came with the calibrated model?

(5) The Authors did not respond to this very important comment. If I did not express myself clear, then I would offer explanation, if the Authors would like to contact me through the Handling Editor. It is important for the validity of the results and any comparisons in their discussion to have such uncertainty estimate of the estimates of the trend slope. It is important to the transparency of the communication to use generally accepted statistical terms. It would be advantageous to know why the Authors choose the specific trend forms (linear, quadratic). I do not think that fulfilling this suggestion would be very time consuming.

(6) Thank you. The request was to put uncertainty bounds on the estimated trends. I am looking forward to seeing the new illustrations including the trend uncertainties

(7) While the title may be consistent with the goal of the paper, in the current form it is not in my view consistent with its contents. I hope that this will change in the revised version.

Thank you for addressing points (8) and (9)

Best wishes, –

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