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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A robust calibration approach for PM_{10} prediction from MODIS aerosol optical depth" *by* X. Q. Yap and M. Hashim

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I thank the authors for the interesting paper. The subject, i.e. the prediction of PM surface concentration from satellite observations, is a very important issue.

I have some remarks which may strongly improve the paper.

First remark

Equation (1) can be re-written as

$$E(Y)_{mn} = \alpha_{\text{fix}} + \beta_{\text{fix}} AOD_{mn} - \beta_{\text{fix}} \varepsilon_{mn} - \beta_{\text{fix}} \varepsilon_{\text{fix}} \quad (1)$$

so you have two 'fix' coefficients: α_{fix} and $\beta_{\text{fix}} \varepsilon_{\text{fix}}$. As I can understand, they are not site/season dependent. How can your statistical approach discriminate between two constant terms? Why not to write this equation more clearly? For example as: $E(Y)_{mn} = \alpha_{\text{fix}} + \beta_{\text{fix}} AOD_{mn} + \varepsilon_{mn}$ where α_{fix} and β_{fix} model the large-scale and season-independent additive and multiplicative bias, and ε_{mn} is a season- and site-dependent bias. Please, could you better explain your statistical assumptions?

Second remark

Equation (2) is written as:

$$\xi_{\text{fix}} = \alpha - \beta \left(AOD_{mn} \right) \quad (2)$$

This is the first time you introduce the 'random effect' ξ_{fix}. How does it relate to the previous coefficients in equation (1)? Moreover you use the subscript 'fix' on the left side and the subscript 'mn' on the right side. I think that this use of subscripts is misleading. How a 'fix' effect could be related to a season- and site-dependent AOD value? Are the α and β coefficients in equation (2) related to the α and β coefficients in equation (1)? There is no subscript for these coefficients in equation (2); are they different from α_{fix} and β_{fix}? Please, clarify these points.

My general impression is, though the subject is very important and results are impressive, the statistical approach is not clearly written, and this may prevent the possibility for other people to reproduce your approach. In my opinion a better and more clearly description of your approach is highly desirable.

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Finally, I have a question about the applicability of your approach for a different time window. You concentrated your attention on monthly averages. Does your approach preserve equally good correlations for daily predictions? Epidemiological studies could be equally applied for short-term exposure effects, so it is highly desirable to downscale your approach until a site-specific daily forecast, if possible.

Best regards and thanks for the attention devoted to my questions.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31483, 2012.

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Interactive Comment