

Interactive comment on “Nonlinear relationships between atmospheric aerosol and its gaseous precursors: Analysis of long-term air quality monitoring data by means of neural networks” by I. B. Konovalov

I. B. Konovalov

Received and published: 4 April 2003

I would like to thank the Reviewer of my paper for generally positive evaluation of my paper and useful comments. The critiques concerns (1) the choice of TSP as an indicator of atmospheric PM, and (2) insufficient justification of the configuration of the neural network models.

My point-to point response to the critical comments is given below.

1. On the one hand, I agree that TSP is a rather poor surrogate for PM₁₀ and PM_{2.5}, which are more closely related to health and visibility effects, although TSP is sometimes indeed used as an indicator of potential harmful effects associated with the urban

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

aerosols [e.g., Delucchi et al., The health and visibility cost of air pollution: a comparison of estimation methods, Journal of Environmental Management, 64, 139-152, 2002]. I have expressed similar opinion in Introduction of my paper (page 839, lines 8-10), and an excuse has been given there that such analysis could not be performed due to lack of observational data (ibid, lines 10-13). The constraints to applicability of the discussed methodology due to insufficient amount of observational data have also been noted in Conclusion (page 857, lines 23-29). Thus, my choice of TSP as an indicator of PM was enforced due to the lack of necessary data for modeling PM₁₀ or PM_{2.5}. And, accordingly, to repeat the analysis with PM₁₀ or PM_{2.5}, as suggested by the reviewer, is impossible, at least, for the same region and for the same period.

On the other hand, I believe that the use of TSP is quite legitimate in the context of the goals and the methodology of this study. Specifically, TSP is well suited to the main goal of my study - to investigate nonlinear relationships between atmospheric aerosols and its precursors, - as TSP encompasses virtually all aerosol components which may exhibit such relationships. (Such components are, predominantly, of secondary origin.) Moreover, taking into account that the secondary particulate matter contributes, mainly, to fine fraction of aerosols, it may be reasonable to expect that similar features of the relationships would be obtained with PM_{2.5}. However, validity of last expectation is yet to be checked in future investigations. As for concentration of primary aerosols which also contributes to TSP, it may hardly demonstrate any regular dependence on concentrations of gaseous aerosol precursors, except trivial linear correlation which is not in focus of my study. In particular, while the cement dust contributing to TSP may, potentially, deteriorate the overall quality of our empirical models, it, obviously, cannot be responsible for appearance of those nonlinear features of relationships between TSP, NO_x and VOCs, which are discussed in the paper.

The justification of the use of TSP as an indicator of atmospheric PM will be extended in the Introduction of the revised version of the paper in accordance to the above commentaries.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

2. I feel that the discussion of the choice of parameters of the model configuration was indeed insufficient. It will be extended in the revised version of the paper. The choice of those parameters is dictated by the necessity to avoid two dangerous possibilities, such as (i) the possibility that the model results are dependent on the particular initial guess for the neural network weights and on particular choice of neural network configuration (this possibility has been discussed in the paper), and (ii) the possibility that the models got undertrained because they are adjusted to bad local minima of the cost function (I am sorry that discussion of this possibility has indeed be missed). The first danger is avoided by means of averaging outputs of the large enough ensemble of individually trained networks. As it is pointed out in the manuscript, this averaging is performed over 50 (in total) networks with different numbers of neurons ranging from 21 to 30. Figure 1 bears strong evidence that the so averaged model outputs are almost "invariant". The second dangerous possibility renders necessity of selection of really well trained networks. As it was properly noted by the Referee, 5 best networks were selected among 50, for each network configuration considered. The incorporation of all trained networks into the model would make the overall results of the study somewhat different, and I believe that, due to the reason pointed out above, those results had to be considered as being less proper and reliable. However, I especially checked if the model results are strongly dependent on the choice of the number of "best" networks. The answer was "no": the evaluations of quantities of interest remained almost the same when this number was equal to 3, or 5, or 10. Anyway, if the model outputs were indeed sensitive to the parameters of the model configuration, it would be almost incredible that correlation between results of entirely different sets of neural networks was so good as demonstrated in Fig.1.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 835, 2003.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)