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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Use of neural networks for tropospheric ozone time series approximation and forecasting – a review" *by* A. A. Argiriou

**A. A. Argiriou**

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My comment addresses the questions / criticism of anonymous referee #2, since the anonymous referee #1 accepts the paper as it is.

The numbers that follow correspond to the numbering used by referee #2 in his comments / observations.

1. The description of NN models (section 2 of the paper) has been intentionally brief and has been limited to the minimum required to remind to the reader the notions and terms used in the review paper. I made this choice believing that a reader interested in such a specific topic, must be already familiar with the basic concept and notions of neural networks and, because of this, a more detailed description would not be necessary. However for the reader seeking further information, I state in the text that



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there are very good introductory textbooks on neural networks and I provide references to the book by Kartalopoulos (1996) for example and to the excellent review paper by Gardner and Dorling(1998) (references as in my paper) that provides a comprehensive description for the multilayer perceptron, i.e. the NN typology mainly applied in all papers that I reviewed. I could, however, either to add in this section of the paper further references to basic textbooks and papers or to expand this section by providing a more detailed description. Therefore I would be grateful if the handling editor and / or the reviewers could advise me on the way to follow.

2. I do not share the point of view that there is a general lack of my own thoughts on the large number of studies and papers reviewed. Before providing a more detailed comment, I would like to state that all the works I reviewed were papers published in refereed scientific journals. No studies were included in my review. Regarding the essential part of the reviewer's comment, the approach used in review papers in general, consists to comment and make an assessment after the presentation of each reviewed item. I chose another approach that consists to firstly point out the general features of each reviewed paper and once all reviewed papers presented and commented to make overall comments in the Conclusions section of the paper. Please allow me to present some examples: I point out for each one of the papers whether the developed NN model is really a forecasting model or a tropospheric ozone concentration time series approximation model. When studying my material, I realized that this is an important issue to point out and clarify each time, since in many papers that I reviewed the word prediction is used, not always in the forecasting sense that one may have expected, but in the function approximation sense. Also, I point out for each paper, if the parameters used as inputs to the NN model are selected via an "objective", i.e. mathematical, method that would allow the avoidance of using as input two strongly cross-correlated parameters, or based on physical chemistry reasoning or, in some cases, in mere intuition. Also I point out each time whether the values were normalized, and within which range, as good NN application practice suggests, or not, depending on whether this information is provided in the reviewed paper. As a final example showing the inclusion



ACPD

7, S4295–S4297, 2007

of my personal point of view allow me to present the fact that for each paper I discuss whether the proposed NN model is assessed adequately by the use of statistical indicators, or if a simple correlation coefficient is used or, there were such cases also, a simple graphical comparison is performed. In the conclusions section I proceed to overall comments where I stress in several occasions the weak, according to my scientific point of view, points of the reviewed papers and those points that may lead other users of the proposed models to confusion. However, I could consider any changes that the handling editor may suggest.

3. I agree with the referee that some consideration should be given to other methods used for ozone time series approximation and forecasting and I could add further information on the issue in a revised version of my paper, if the handling editor agrees.

4. I agree with the referee that the issue of the time series autocorrelation and how do NNs handle it is important and needs to be further discussed and I could do this in a revised version of my paper.

5. I agree with the referee that further discussion is needed on the points that he raises. Again I can add such a discussion in a revised version of the paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 5739, 2007.



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7, S4295–S4297, 2007