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Hydrology and Earth System Sciences Discussions

Interactive comment on "Generalized versus Non-Generalized Neural Network model for multi-lead inflow forecasting at Aswan High Dam" *by* A. El-Shafie and A. Noureldin

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We would like to thank the referee for his objective and thorough review of our paper. We have addressed all the referee's comments in the following point-by-point response. All changes made to accommodate the referee's comments are underlined in the revised manuscript.

This paper compares a standard (non-generalized) artificial neural network (ANN) model with a generalized ANN model for inflow forecasts at multi-lead times. The models are tested using inflow data from the Aswan High Dam located in Egypt. Overall, I

think the paper is well written and the authors address an important topic in hydrology (inflow forecasts) that is of keen interest of the Hydrology community. However, based solely on the paper results, I am not fully convinced that the proposed model has much advantage (if any) over the classical AR and ARMA (including periodic AR) classes of inflow forecast models. Some results are also very confusing and may lead to wrong conclusions. Moreover, the apparent higher forecast skill of the generalized

ANN based model over the standard ANN model obtained by the authors is somehow expected and do not bring anything new to the hydrology community. Hence, I do not recommend this paper as it is for publication at the Hydrol. Earth Syst. Sci. Journal. I think the paper needs major revisions in order to be considered for publication. Major and minor comments to support my view are listed below.

Note: Once I finished this review I realized there was another review posted by Referee #1 and a reply from the authors. As expected, there are some overlapping comments in this review and I decided not to remove them.

Reply

The authors appreciate the opinion of the referee on our research and thank him for his positive comments that will strengthen the content of our research manuscript.

The authors review the whole comments; we can observe that some of them are related to the ARMA model which was used to perform the comparison analysis with the proposed ANN model. The authors would like to highlight here and before starting preparing the reply to the comments, that the ARMA model was NOT developed by the authors, but it was developed by (Salem and Dorrah, 1982) and several modifications had been applied and added to the original version, the authors used the last version updated on 1998.

Unfortunately, those modifications had not been published anywhere, but only used within the Nile Water Sector, Egypt. On the above of that, the source code of this model is NOT available at the author's side to perform any modifications as recommended by the referee; however, the authors provide an explanation (in the revised version of the manuscript) about the model based on the last modification applied to the model in 1998.

The author thanks the referee for the note, in this context; the authors provide similar reply for the overlapping comments with referee #1.

Major Comments

Eqs. (1), (2) and (3):

a) Historically, the function f in these equations has been well represented by linear functions (just look at the success of the classical auto-regressive based models). In general, the inflow at time t is linearly related with past flows as shown by the (periodic) auto-correlation function of several inflow series. Hence, the authors do not take advantage of the main feature of Artificial Neural Network (ANN) models, which is their ability to map non-linear, complex relationships between input and output data.

Reply

The authors agree with the referee that, historically the AR models with its linear function showed a successful performance in treating such forecasting functions. In some cases, such as the one under this study (Aswan High Dam), the existing ARMA model showed relatively weakness in the accuracy level as presented in the comparison analysis.

In fact, the river inflow is stochastic in nature and the interrelationships in the inflow pattern between two consequence months are non-linear. The proposed ANN model with its structure including the transfer functions between the layers, the number of neuron within each layer, the weights associated with its neuron create a complex non-linear function between the input and the output. The bias associated with each neuron treats the stochastic components of this relationship between the input and the output.

In our case study, the proposed ANN model architecture showed ability to detect such non-linearity and the stochastic behavior in the inflow pattern.

b) I am not convinced that a third order model (i.e., the inflow at time t-3 still has some influence on the inflow at time t) is physically plausible for this problem, especially for the wet period months. Unfortunately, ANN models are black boxes and cannot show whether such persistence relationships expressed by model parameters are significant or not.

Reply

Basically, the proposed neural network model in our study is NOT mainly rely on the physical and/or hydrological behavior of the system in the study area, it is conceptually a time series forecasters with consideration of the inflow pattern of consecutive months. To predict the time series of different systems' behavior that uses the previous and most recently behavior of a system to predict its future changes. The major advantage of this method is the ability to predict the behavior of systems without fully consideration or analytical prediction rules (hydrological/physical). As a result, within this concept, any month could be forecasted as long as the previous data records could help enhancing the forecasting skills and are available in the time series.

c) If the interest is to predict Q(t+1)- Eq. 2, why not just use (Qm(t-1), Qm(t-2), Qm(t-3)) as predictors rather than first predict Qf(t) with (Qm(t-1), Qm(t-2), Qm(t-3)) predictors then use (Qf(t), Qm(t-1), Qm(t-2)) to predict Q(t+1)? I think the recursive (or plug-in, iterated) approach employed by the authors is more appropriate for AR based models and the authors may get much better results if they use a direct approach as suggested. I think they should at least compare both approaches.

Reply

Keep in mind that our proposed forecasting model is to have 12 neural networks model one for each month. This is because each month has particular pattern different than the others. The findings of the cross-correlation analysis between two consequences months shows that the cross-correlation is relatively poor if go more than 3 months behind the one to be forecasted for most of the months. Based on that observation, theoretically, for the multi-lead forecasting, the use of the forecasted value as presented in equations 2 and 3 (now in equations (2.a), (2.b), (3.a) and (3.b) rather than using the actual value of the months (t-1), (t-2) and (t-3) "which is 4 month behind (fourth order) in case of forecasting Q(t+1)" to forecast the value at Q(t+1) is more logic (as long as it is highly correlated with the output) to provide more accurate pattern as input and thus more accurate output, even the value of the Qf(t) has certain level of error. Owing to the referee feedback, the authors add a comparison analysis between these two approaches using the ANN model.

Figures 7, 8 and 9: NOW figures 11, 12 and 13 a) The authors should clearly identify the lead time of the forecasts (I presume it is one month) – Lines 19-26, page 7969.

Reply

This paragraph has been modified to clearly identify that the results showed in figure 7, 8 and 9 are for ONLY first multi-lead ahead (L=1).

b) There are clear evidences of a strong bias in their models. Figures 7a, 7b, 7c, 8c, 8d, 9b and 9d show that the model is constantly overestimating the observed values while Figs. 7c and 8b show evidences of underestimations. What might be the cause of that?

Reply

Actually, in developing such forecasting model using Neural Network, the error level is the most important indicator of the successfulness of the model. The proposed model use the back-propagation algorithm during training in order for searching the optimal values of the weight and bias associated with each neuron that the difference between the target "the actual inflow" and model output is minimal without consideration whether

the difference is above or below zero "over-estimate or under-estimate"). Once the error level is acceptable, the bias level (over-estimate/over-estimate the actual values "±error%") might be in minor importance in evaluating the model performance. In this context, in this study the authors used two statistical indices (Root Mean Square Error and Maximum Error) these performance indices to make sure of that the proposed model could provide consistent level of accuracy during all periods. The advantages of utilizing these two statistical indices as a performance indicator of the proposed model are as follow:- Using the maximum error is to make sure that the highest error while evaluating the performance is within the acceptable error for such forecasting model. While utilizing the Root Mean Square error is to ensure that the summation of the error distribution within the validation period is not high. Consequently, using both indices is guaranteed consistent level of errors which is providing a great potential for having same level error while examining the model for unseen data in the testing period.

c) In addition to these figures, it would be interesting to see some figures of actual forecasts versus observed values.

Reply

Owing to the reviewer feedback, two more figures have been added showing the actual forecasts versus observed values.

Lines 9-18, page 7972 and Table 4:

a) The ARMA model and associated parameters used by the authors should be better explained. ARMA models are usually referred as ARMA(p,q) to indicate the number p of autoregressive terms and the number q of moving average terms – What did the authors use?

Reply

Explanation about the ARMA model used in this study now added in the revised version of the manuscript.

b) Since inflow series are usually marked by a distinguished seasonality, it is common in the literature to employ periodic auto-regressive models in order to better address the seasonal variation in the auto-regressive parameters (usually dry period flows show a slow decay in the periodic autocorrelation function whereas wet period flows have a faster decay in the periodic autocorrelation function). The authors themselves fit 12 ANN models (one for each month) in order to better capture such seasonality. Hence, I do not think it is a fair comparison (12 ANN models versus an ARMA model) and I am very confident that the authors would obtain much better skills in their forecasts if they had used a periodic ARMA or periodic AR model rather than an ordinary ARMA model.

Reply

The authors fully agree with the referee in this comment and with his opinion that if the existing ARMA model re-structured as the proposed ANN model (periodic ARMA model) could provide better results over the reported one in the manuscript. Unfortunately, the source code of the ARMA model is NOT available at the author's side to be able to make such modifications. In addition, such modifications in the ARMA model are not within the scope of the manuscript.

c) The relative error RE is for only two hydrological years (1998-2000), which is a too short period and has not enough data to draw solid conclusions on model forecast skills.

Reply

Owing to the referee feedback, the authors extend the comparison analysis five hydrological years between 1995 and 2000.

Table 5: This table as well as the conclusions associated with it (lines 19-27, page 7972 and lines 1-5 on page 7973) are very confusing and hard to understand. The authors should either clarify or completely change it.

Reply

This table now is Table 6 in the revised version of the manuscript. The authors agree with the referee, a re-arrangement for table 5 has been done for better understanding. Also, the authors find out to introduce the the suggested recommendation by the referee reported in the first comment [Eqs. (1), (2) and (3); C] in the same table.

Minor Comments

- Line 15, page 7958. Where is Lake Nasser? The authors should mention where it is located.

Reply

Added information about the location of Lake Nasser has been added.

- Line 1, page 7959. The acronym ANN should be defined here.

Reply

Done

- Lines 2-3, page 7964. What is the range in the number of hidden layers and in the number of neurons tested?

Reply

Modification to this line has been done to show the range of hidden layers and the associated number of neuron tested. In addition, sensitivity analysis for this issue has been discussed in the result and discussion section.

- Line 3, page 7966. Is really Eq. (10)? Maybe Eq. (4)?

Reply

Corrected, in the revised version of the manuscript Eq. (6)

- Lines 2-3, page 7970 and Table 2. December and June do not show small RMSE values.

Reply

Correction to the this statement has been done, to change these two months with the other more smaller RMSE values associated with November and march

- End of Line 18, page 7970. Replace Column 3 by Column 5.

Reply

done

- Line 8, page 7972. As far as I understand, the results do not refer to forty years of data.

Reply

Your observation is correct, an added table (4) to show the RMS for the forty years between 1961 and 2000.

- There are a few typos in the text that the authors should address.

Reply

Review of the typo errors has been corrected.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7957, 2010.