Hydrol. Earth Syst. Sci. Discuss., 6, S389–S401, 2009
www.hydrol-earth-syst-sci-discuss.net/6/S389/2009/

Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Daily reservoir inflow forecasting combining QPF into ANNs model" *by* Jun Zhang et al.

Jun Zhang et al.

Received and published: 23 March 2009

The authors wish to thank referee #1 for spending his/her time and addressing his/her insightful, rigorous and constructive comments which will improve the quality of this manuscript.

Before Addressing responses to two referees, we clarify two important points about this manuscript. Firstly, the main aim of this manuscript is an application of ANNs considering QPFs for mid-term inflow forecasting with lead times of several days in China and this aim, in our opinion, belongs to 'Hydrology and Engineering Applications' and is within the disciplinary fields of HESS. Secondly, the objective of our research is interval inflow forecasting of reservoirs rather than total inflow forecasting, so outflows from the upstream hydroelectric plants are not considered in this manuscript.

All comments given by referee #1 will be considered carefully and most correction requests will be adopted in revised manuscript. Detailed responses to all comments are as follows:

1. General Comments:

Comment:

The paper describes an application of the Artificial Neural Network (ANN) model utilizing Quantitative Precipitation Forecasts (QPFs) for producing 1- to 7-day ahead inflow forecasts. For this purpose, the authors have applied a 3-layer ANN structure involving Back-Propagation (BP) learning algorithm and incorporating a self-adaptive training scheme with adapting learning rate and momentum term. The authors have used up to 3-day ahead QPFs, available from a medium range Numerical Weather Prediction system, to forecast reservoir inflows for 'operational planning and scheduling of hydroelectric power system' involving reservoirs.

In my opinion, the application of a rather conventional ANN model to 'river-flow forecasting' is the only contribution to the journal or researchers. The paper has a number of shortcomings described in the following section. Unless these shortcomings are addressed, the paper may not be suitable for publication in the HESS Journal. Except for the requirements of reorganising some details and providing some additional information as indicated in section 3 below, the paper is generally well organized. The language is mostly understandable, but would require editing before publication in the journal; the authors may consult an English editor for this purpose.

Response:

All shortcomings will be addressed in detail in this response and all correction requests will be adopted in revised version of this manuscript.

2. Shortcomings:

Comment:

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i) From the section on 'Study area and data collection' and Fig. 1 it appears that Shuikou reservoir is located on the Minjiang River and that a number of reservoirs, presumably being used for hydropower generation, exist upstream on the main river and its tributaries. It is therefore highly likely that regulated discharges from all these upstream reservoirs and associated hydroelectric plants have considerable impacts on the pattern of inflow to the Shuikou reservoir and that the inflow to the Shuikou reservoir is highly variable in time.

In this context, I feel that, although the inclusion of one antecedent daily discharge on the basis of ACF as input to the selected ANN models may have implicitly accounted for a component of this variability, the highly non-parsimonious ANN structures have resulted because of the attempts to over-fit the highly variable observed flow data. It is noted that, with the 6-12-1 and 8-20-1 structures of the authors' Model(t+0) and Model(t+2), the weights (including bias) of the resulting ANNs are 97 and 201 respectively. These are undoubtedly very large numbers. Because of the lack of parsimony, the resulting models are likely to be very unreliable for real-time forecasting, particularly for input data which may not be within the range of data used for training the networks. An indication of this may be found in the results of ARIMA and Model(t+0) in Table 1, which shows that, despite a relative improvement in CE and R2 in verification in the case of the simplistic ARIMA model,there is a reduction in performance in the case of the corresponding (if I am right!) non-linear ANN Model(t+0). Also, from the scales provided in Figs. 5(e, f and g), it can be seen that some of the discharge values, simulated in the validation phase, deviate considerably from the 45 degree line when measured in the unit of flow used, i.e. cumec. In view of the above, I request that the authors include some details of the upstream reservoirs, e.g. locations, size, mean daily outflows etc., in a tabular form to provide a holistic picture of the hydrologic system that they are modelling. They should also explain their considerations in respect of accounting for the likely variability of the inflow to the Shuikou reservoir caused by the outflows from the upstream reservoirs or hydroelectric plants. I believe that, without these explanations, the contents of the paper a) do not reflect what the title of the paper says,

b) report the outcome of a typical (and trivial in the sense of automated application of the ANN structure) river flow forecasting only and c) does not merit classification as a good research publication.

Response:

In this manuscript, the forecasting objective we focused on is interval inflow rather than total inflow of Shuikou reservoir, and the total inflow is considered only in the multireservoir system operation phase.

Regarding the nodes number in hidden layer of ANNs, there is no rule of thumb to specify the appropriate number of neurons in the hidden layer (Shamseldin, 2002). We agree with Referee #1 that it is a very interesting research of reducing the nodes number in hidden layer and it is worth of a further study. However, we think this is not so crucial in some applications of ANNs, especially the applications oriented to practical engineering. Jain and Indurthy (2003) used two ANNs models, one of which is single hidden-layer ANN model with structure 10-20-1 while another is multiple hidden-layer ANN model with structure 10-12-14-1, to perform event-based rainfall-runoff modeling.

Comment:

ii) Descriptions and the notations of the ANN models in subsection 3.1 are not clear. It appears that the forecast time origin is t-1, so that the 1 day ahead forecast is indicated as being Q(t+0), i.e. Q(t), the corresponding model being represented by Model(t+0), and that the models have been used in non-updating mode. In this context, Q(t+1) in expression (1) (line 16, page 128, indicated hereinafter in this review by the convention 16/128) should be Q(t+0) and Model(t+0) in expression (4) (16/128) should be Model(t+i). The authors indicate that 'no QPF more than three days are available at present'. For forecasting 'next four days' inflows, they state that Model(t+3) is 'a unified model' given by expression (4). What do the authors mean by the term 'unified model'? The expression for this 'unified model' includes QPF(t) = 0 but does not adequately indicate that the QPFs at times t+3, t+4, t+5 and t+6 are unavailable. What are the inputs to these ’unified models’? How have the authors used the QPFs available for the previous three days? The forecast time origin remaining the same, have the authors consistently used the inputs P(t-2), P(t-1) and Q(t-1) in the models for 4-, 5-, 6- and 7-day ahead forecasts, these inputs being common to the models for 1-, 2- and 3-day ahead forecasts? It is also not clear from subsection 3.2 if the ’unified models’ have 12 neurons in the hidden layer like those in Model(t+0). Please clarify. Expressions, similar to those in (1), (2) and (3), will be useful. In the above context, it is also noted that the graphical displays of all outputs correspond to Model(t+0), Model(t+1) and Model(t+2). No result is provided for Models(t+i), i = 3, 4, 5 and 6. It would perhaps be better to drop all references to Models(t+i), i = 3, 4, 5 and 6 from the paper, and rather concentrate on those models for which QPFs are available for use.

Response:

Actually, the QPFs information is released at 08:00 a.m. every day as we described in the manuscript (see line 18, page 126, indicated hereinafter in this response by the convention 18/126). The forecasting is automatically carried out after this release in time and the forecasting results are delivered immediately to the ’operation department’ of Fujian Power Grid Company for daily planning and scheduling of hydro-electric power system based on a multireservoir system optimal operation calculation. Because scheduling is for next 7 days including ’t+0’-’t+6’ while the QPFs information is for next 3 days including ’t+0’-’t+2’, we designed four models named model(t+0), model(t+1), model(t+2), model(t+3) to use different QPFs information for the forecasting. Forecastings for next ’t+0’-’t+2’ are based on model(t+0), model(t+1) and model(t+2) respectively, and they were discussed and analyzed in detail in the manuscript. Forecastings for next ’t+3’-’t+6’ are based on model(t+3) which has the same structure as model(t+0) and they were not the emphasis in this manuscript duo to the lack of QPFs information for these days though they were implemented in the software system. So, results and discussions of forecastings for these days are not included in this manuscript. The forecast time origin is not ’t-1’, but ’t’. Q(t+1) in expression (1) (see 16/127) should be

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Q(t+0) and this will be corrected in revised version. However, Model(t+0) in expression (4) (22/127) is correct and this expression means that model(t+3) which has the same structure as model(t+0).

Comment:

iii) The authors have used the mean of the previous 30 days observed rainfall and flow to incorporate 'the seasonal information'. Why 30? Do the observed rainfall and discharge display any seasonality? If yes, then a graph to display the seasonality, e.g. by plotting the means of rainfall and discharge at each day over the number of years for which data have been used, will be useful. Although seasonality can be expected in the rainfall data series, I am not sure if the discharge data series, influenced by regulated outflows from upstream reservoirs or hydroelectric plants, will display marked seasonality. Authors need to clarify this aspect.

Response:

Considering our forecasting horizon is within mid-term including several days, mean values of observed rainfall and discharge of too long days are not necessary. Based on the basin characteristics, '30 days' was determinated by the forecasting operators in Fujian Power Grid by their experiences. As Referee #1 pointed out, this aspect will be deserved a further study.

Comment:

iv) The bench-mark ARIMA model, having a (4,1,2) structure, has been used in the study. What is the basis of selecting this particular structure? Apparently, this model has been used only for 1-day ahead forecast. Therefore, outputs from Model(t+0) can only be compared with those from the ARIMA model. However from the abstract or from the section on 'Introduction' (10/125), the reader gets an impression that outputs from all selected ANN models (each of which is unique!) have been compared with the ARIMA model outputs. This requires clarification.

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Response:

AIC (Akaike Iformation Criterion) metric proposed by Akaike (1974) is the basis of selecting parameters including AR(p), MA(q) and differencing(d) in ARIMA model (Box and Jenkins, 1976) and necessary description will be added in revised version. ARIMA model is one of classical time series based methods and has been used extensively in a wide variety of forecasting applications including hydrological forecasting, so it is selected as the bench-mark -a simple less complex model suggested by the editor. Regarding the easily confused description, they will be improved in revised version.

Comment:

v) For each of the 2- or more-day ahead forecasts, did the authors try the structure of the model Model(t+0) itself by replacing the observed antecedent flows by the modelsimulated flows and the observed antecedent rainfalls by the QPFs? Such replacements would be required only for the day(s) which lie between the forecast origin and the day for which the forecast is required. It is worth investigating the performance of this model for each of the 2- or more-day ahead forecasts vis-a-vis that of the corresponding model in the set of Model(t+i), i=1,2,3,...6. The ARIMA model can also be used in the same way for producing 2- or more-day ahead forecasts, i.e. using the simulated discharges and the QPFs for the day(s), antecedent to the day for which the forecast is required, but beyond the forecast origin. If used in this way, the outputs of the ANN model may be comparable to the outputs of the ARIMA model, although, strictly, these will not satisfy the criterion of 'like-with-like comparison' because of different sets of inputs being used. If not already done, the authors may apply the Model(t+0), as suggested above, and justify the choice of the model forms finally selected for the study.

In the above context, it may also be noted that, although the performance of a single forecasting model in non-updating mode is generally expected to gradually decrease with the increase in lead-time of forecast, such a trend is not obvious from the values

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of error measures in Table 1 and 2, only because each ANN model, finally selected for the study, is unique. A meaningful comparison is therefore not possible.

Response:

Thank Referee #1 for the good idea about improvement of the 2- or more-day ahead forecasts by replacing the observed antecedent flows by the model simulated flows and the observed antecedent rainfalls by the QPFs, and we will try it in a further study. In the revised version, we will provide a figure about the relationship between the error measures of ANN and the node number in hidden layer to demonstrate the process of model structure determination.

Comment:

vi) The authors have based their selection of antecedent input flows and rainfalls on the basis of the ACF and CCF values. I suggest that for the discharge data series, the authors also provide graphical display of the PACF (Partial auto-correlation function) values to give a better idea of an appropriate ARIMA model.

Response:

The graphical display of the PACF will be added in revised version.

Comment:

vii) It is desired that, for each ANN model, the result of the 'experiment with a trial-anderror measure' (17/128), used to decide about the number of neurons in the hidden layer, be graphically presented to show the relative change in the error measure with the number of hidden layer neurons. This is necessary to justify the authors' choice of 12, 15 and 20 neurons in the hidden layer for the models finally selected for 1- 2- and 3-day ahead forecasts. In this context, authors may refer to Fig. 2 in Toth et al., 2000.

Response:

The graphical display of relative change in the error measure number of hidden layer

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neurons will be added in revised version based on careful study of relevant literatures including Toth et al. (2000).

Comment:

viii) The authors have used an adaptive learning algorithm for training. It would be worth, for the sake of completeness, to include a comparison of the training phase considering a fixed learning rate and a fixed momentum term (e.g. 0.5 and 0.5). For this purpose, the comparison may be drawn in terms of either the number of epochs or the time taken in training a network by both non-adaptive and adaptive procedures.

Response:

The graphical display of comparison between fixed training parameters (including earning rate and momentum term) and adaptive parameters will be added in revised version.

Comment:

ix) The application reported in the paper is for reservoir flow forecasting for 'operational planning and scheduling of hydroelectric power system' involving reservoirs, as distinct from an application for flood forecasting flood. For this purpose, the models, developed in the current study, will be expected to be reasonably good in forecasting flows across a wide range of flow variability, i.e. for high, medium and low flows. Although, it is recognized that no model can successfully simulate both the high and low flows, some indication of the degree of fit of the simulated flows with the observed flows in different ranges of flows will be relevant to the study reported in the paper. The global values of error measures, as given in Tables 1 and 2, are not very useful. It is suggested that the authors produce additional values of the error measures separately either for each of the high, medium and low ranges of flow or for each of the four parts divided by three quartiles of the observed discharge series. Graphical displays of the selected error measures may be useful. In this context, authors may refer to Fig. 3 in Toth et al.,

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2000.

Also, the authors may agree that Figs. 3 and 4 in the paper, meant to visually display the degree of fit of the observed and the simulated flows for three lead-times, fail to serve the purpose. For easy visual comparison, the scales for both the observed and simulated discharges in these figures should be the same. Even with the modification of the scales, the plot area in such a figure will be too small to justify the inclusion of data for the whole calibration or validation period. It is suggested that, for each of the calibration and validation periods, plots showing the degree of fit of the highest flow, the second highest flow, a flow in the middle of the range of flows and a low flow, including, in each case, a few days before and after the occurrence of the particular observed flow considered, be presented for each lead-time. In this context, authors may refer to Figs. 3 and 4 in Goswami et al., 2005. Although it is expected that, for each lead time, the simulated flows may not be able to reproduce the corresponding observed flows in magnitude and time of occurrence of the observed flows, these plots will give a better indication of the degree of fit across the whole range of flows. The author's statement: 'The simulated curves in both Figs. 3 and 4 clearly indicate that not only the rising trends and the falling trends in the hydrograph are picked up by Model(t+0), Model(t+1) and Model(t+2) but also excellent goodness of fit performances are achieved' (26-27/131 and 1-2/132) is very bold and is not enough. Similarly, the statement: 'From the scatter diagrams in Fig. 5, it is obviously that both of the low values and the high values are close to the exact fit line and this result suggests that there is no evident overestimate or underestimate occurs during the simulation' (11-13/132), in addition to being grammatically wrong, is inappropriate.

Response:

Regarding the error measures, there is a general lack of objectivity and consistency, as pointed out by Legates and McCabe (1999), in the way in which rainfall-runoff models are assessed or compared. We designedly choosed 5 presentative error measures to evaluate the performance of the proposed models. Among these error measures, CE is sensitive to extreme values, MSRE provide a more balanced perspective of the goodness of fit at moderate flows, and MAE computes all deviations from the original data regardless of sign and is not weighted towards high flow events. In addition, RVE gives the relative bias of the overall water volume balance of the model (Dawson and Wilby, 2001; Dawson et al. 2007). So, in our opinion, the utilization of these different error measures and the analysis based on them, to a certain extent, can give a relative comprehensive evaluation of the proposed models. Moreover, more concerns, in practical operation management, are about the water volume rather than magnitude of discharge considering regulation and storage capacity of Shuikou reservoir.

With respect to Figs. 3 and Figs. 4, we acknowledge that they fail to serve our purpose duo to too much long period of calibration and validation. Thank referee #1 for the feasible advice of plotting some presentative flow with a few days before and after the occurrence of peak flow. In revised version, we will provide three biggest peak flow in calibration period as well as the most biggest peak flow in validation period to show the goodness of fit performances and some inappropriate description will be corrected.

Comment:

x) The authors must highlight the sources of uncertainties in the inflow forecasts being produced by the selected models. Particularly the uncertainty associated with the estimation of the QPFs may considerably influence the uncertainties in the inflow forecasts.

Response:

We agree with Referee #1 about that estimation of the QPFs may considerably influence the uncertainties in the inflow forecasts and this aspect is very interesting issue deserved investigation. But at present, information about QPFs for us is limited for several reasons. More relevant information will be added in revised version and more detailed analysis will be accomplished in a further study.

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3. Organization of the paper:

Comment:

i) The periods of calibration and validation have been specified in the ’Results and discussions’ section (17-18/131). These specifications should be included in the section describing the methodology.

Response:

The periods of calibration and validation will be moved to the section describing the methodology in revised version.

Comment:

ii) The structure of the ’bench-mark’ ARIMA model has been briefly provided in the ’Results and discussions’ section (19-20/132). However, a separate subsection under section 3: ’Methodology and modelling’, providing the basic description of an ARIMA model, the structure of the model finally selected for the study and the mode of application of this model for lead-time forecasting in the current study, will be desirable.

Response:

Necessary description of ARIMA model will be added after section 3: ’Methodology and modelling’ in revised version.

Comment:

iii) It may be more appropriate to provide the details on ’Software implementation’ in an Appendix.

Response:

We agree with he viewpoints from both referee #1 and the editor about section ’Software implementation’ and this section will be adjusted as an appendix in revised version.

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(to be continued.)

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