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Interactive Comment

Interactive comment on "Constraints of artificial neural networks for rainfall-runoff modelling: trade-offs in hydrological state representation and modelevaluation" *by* N. J. de Vos and T. H. M. Rientjes

Anonymous Referee #2

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General comments

The paper addresses an interesting aspect of the application of rainfall-runoff transformation models based on artificial neural networks (ANN). Having observed a timing error in the hourly data simulation, the authors test several alternatives for the input variable(s) representing the soil moisture condition of the watershed, usually consisting of the past runoff values: the moving average of past rainfall, the moving average of past runoff, the soil moisture state variable used in a conceptual model and combinations of such variables. The results show that the alternative input variables partly correct the timing error, but result in a decrease of the overall goodness-of-fit. The

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Interactive Discussion

Discussion Paper

presented work is certainly novel, suitable for the journal and of international interest, since the optimal selection of input variables is always crucial in the application of ANN and any effort towards the reduction of the simulation timing error is of extreme interest. The paper is well organised, extremely clear and shows a rigorous scientific approach but I do believe that the proposed approach (that is, the replacement of past runoff values with other input variables, in same cases redundant, for representing the watershed condition) is probably not the best solution to the timing error problem, as shown also by the deterioration of the overall fit of the predictions. In my opinion the authors should focus on the other methodology cited in the paper, but unfortunately not applied to the presented case study, that is the on the use of multiple objective functions in the model calibration, with the objective of giving more weight to the timing error. Since the paper does not describe the application of this second approach, I would suggest to remove its description from the abstract, the introduction, section 4.1 (lines 8 to 12 of page 382) and the conclusions (at least not referring to it as a conclusion of the presented work but rather as a future research field), otherwise such paragraphs might misleadingly induce the reader to think that such approach is actually tested in the presented work. In addition, the application to daily data (not including the test of different input variables, which is the heart of the work) does not seem indispensable.

Specific comments

p. 376, ll. 13 to 22: I would posticipate the description of the selection of the number of hidden nodes in section 3.4 (here it is not, in addition, specified to which application - daily or hourly data - nor to which training algorithm the selection refers to); p. 377, l. 7: “a = net”? ll. 18-20: it does not seem appropriate to justify such an important modelling choice on the base of personal experience, not referring to published works (or, better, providing the results of a comparison with other input sets). ll. 23-25: the description of the selected delays should be moved here from p. 379. p. 378-379: I would suggest to merge the three paragraphs from l. 8 to l. 11 of p. 378, from l. 15 to l. 19 of page 378 and from l. 6 to l. 13 of p. 389. p. 379, l. 19: please justify the choice of a time memory

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of 5 steps for the evaporation when the correlation seems almost flat in Fig. 7. l. 24: please specify how the catchment mean lag time is obtained. ll. 22 to 24: actually, the fact that the mean lag time is so high in comparison to the lead time would advocate a more than reasonable forecasting accuracyĚ p. 380, ll. 18-19: the multistep ahead forecasts are "disappointing" or, as said at l. 11, "reasonably good"? p.381, ll. 3-4: actually, in Fig. 14, the major disagreement between the 6-h lead-time forecasts and the observed series does not seem the timing error. ll. 8-9: I would suggest putting off the introduction of Fig. 15 after the description of the procedure of time shifting (ll. 11-13). An interpretation of the results shown in Fig. 5 is also recommended. p. 383, l. 15: please justify the choice of 192 h for the memory length of the moving average runoff. ll. 19-20: a (synthetic) description of the soil moisture component of the GR4J model is here needed. Such description may also help to understand the comment (p. 384, ll. 21-22) regarding the possible reasons for the abrupt changes in the recession curves obtained when using the SM series as input. p. 385, ll. 3-4: please justify the deduction that the information from the input signals is equally weighted.

Technical corrections

p. 373: the PI index is introduced at the end of the section, seeming a subsequent addition, whereas the other two indexes are indicated at the beginning of the section as "the most important measures": I would give the same importance to the indexes. p. 373, 377 and 378: I suggest removing the references to Zijderveld (2003) and De Vos (2003) because a reference to not easily accessible papers does not seem necessary when describing general properties of ANN often cited in the international literature. p. 375, l. 7: evaporation or evapotranspiration? p. 380, l. 18: the indexes are described more completely (that is along with the mean and standard deviation values) in Table 2 than in Fig. 11 (where, by the way the PI index is 0.13 and not 0.121 as indicated in Table 2).

Caption of Fig. 13: it should be added that the figure refers to the hourly data application. Figures 16 and 17: the simulation with input (P + Q) should be added, for comparison (and the hydrograph shown in Fig. 14 is of no use since it refers to a different time period).

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