Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-610-RC3, 2020


Hydrology and Earth System Sciences
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Discussions

# *Interactive comment on* "Multi-step ahead daily inflow forecasting using ERA-Interim reanalysis dataset based on gradient boosting regression trees" *by* Shengli Liao et al.

**Anonymous Referee #3**

Received and published: 29 January 2020

In this manuscript, the authors compared several data-driven models for multi-step forecasting of inflow. The employed models include gradient boosting regression trees (GBRT), artificial neural networks (ANN), support vector regression (SVR), and multiple linear regression (MLR) models. The models were developed by considering (1) streamflow and rainfall record, and (2) ERA-Interim reanalysis data. Further, the maximum information coefficient and autocorrelation functions were utilized to construct the input structures of the models. The authors concluded that the developed methodology that considers ERA-Interim reanalysis data considerably gives better results in the forecasting of inflows at lead times of 5-10 days. The manuscript is well written and organized. However, there is not a significant novelty in the manuscript except using



ERA-Interim dataset. Further, there are severe weaknesses in the developing of the model input structures. Please see my comments below.

1. The authors made a significant mistake in using the autocorrelation function (ACF) in determining the model structures. They should have employed cross-correlation and partial autocorrelation functions (or other measures) to establish the relationship between the observed records and inflow. The ACF only measures the dependency or relationship of observed value with lagged observations of a considered variable. In a long-dependent series such as inflow time series, the ACF will decay slowly. Therefore, statistically significant relationships between the observed and lagged values could not be determined. To determine the significant relationships, the authors employed user-defined threshold value. The obtained inflow and rainfall values for the input structures of the models include only three lagged-day values as could be expected. This number could be higher based on the selected threshold. However, this finding does not convey any meaningful relationship between the observed records (i.e. inflow and rainfall) and the inflow values. The PACF should have been used for determining the lagged relationships of inflows since the inflow time series mainly shows the long-memory feature where the correlation decays after a long observation period. Further, all statistically significant lagged variables should have been included in the model structures found in PACF. Using a user-defined threshold value is a serious mistake in this situation. 2. The authors claimed that the proposed methodology "significantly" improves the accuracy of inflow prediction for longer lead times. However, I do not agree with this comment. Because, as the authors mentioned, there is only about 1% and 5% improvement in two-day and 10-day ahead forecasting. Therefore, the results do not seem convincing about the superiority of ERA-Interim dataset over the common dataset, especially ill-conditioned input structures with conventional observed inflow and rainfall dataset. 3. The authors found that three-day lagged values of inflow and rainfall have less impact on 10-day ahead forecasting of inflow in Section 4.5. This is a clue that more lagged values of input variables should have been included in the models' structure. 4. The employed performance indices, specifically the coefficient of determination, seems

insufficient to compare several model performances. More distinctive performance indices such as degree of agreement and Kling-Gupta efficiency metrics should have been used. 5. It is not clear how the multi-step forecasting scheme (i.e., recursive or static) was employed? Please give more details about this issue. 6. The selected ranges of the model parameters seem highly subjective. Please justify the selected ranges of the model parameters, especially in Section 4.2. 7. The range for the number of hidden neurons (i.e. 2–20) seems too high. Please justify this from a hydrological perspective. Because using a high number of hidden neurons could lead to overfitting that resulted in a poor performance in multi-step forecasting. 8. The discussion of the obtained results should be improved with more details, especially giving necessary citations to previous studies. 9. It is not clear how Fig. 1 was obtained. Please give the necessary information about this figure. 10. Please give more details on the Lines 78–82. 11. Please give the definitions and meanings of the variables in the ERA-Interim dataset in the Appendix. 12. Please justify using the feature scaling in Line 108. 13. What do you mean with “invalid variables” in Line 116? 14. Please prefer “maximal” or “maximum” information criterion throughout the manuscript. 15. Please check the term MI*(D,X,Y) in Eq. (5) since you defined MI*(D,x,y) in Line 130. 16. The definition of B(n) was given in Line 133; however it is not clear where this parameter is used. 17. Please check the terms in Eq. (7). Will they be R1(i,s) or R1(j,s)? 18. Please check the notations in Line 144; n features with N samples or n samples with N features according to the given definition. 19. There is little information about the structure of ERA-Interim dataset. Please give more details about this dataset. 20. There is not any information about grid searching methodology. 21. Please add “activation function” after “relu” in Line 248. 22. The comments in Lines 278–280 are vague. 23. The authors did not discuss the reasons why NSE values for lead times of 6-7-8-9-day is worse than the value of lead time of 10-day. 24. It is not clear how top k features were selected according to the chosen threshold value. Did the authors employ several threshold values? Please give more details on this issue.



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