

Interactive comment on “Empirical streamflow simulation for water resource management in data-scarce seasonal watersheds” by J. E. Shortridge et al.

Anonymous Referee #2

Received and published: 25 November 2015

General comments

The manuscript describes an hydrological modelling problem – the identification of different data-driven (machine learning) models for five rivers in the highlands of Ethiopia. The study is not limited to a streamflow prediction problem but also includes (a) a comparison against a process-based hydrological model, (b) an analysis of the models structures and residuals, and (c) an analysis of the uncertainty associated with predictions under various climatic conditions. The overall objective is to highlight strengths and limitations of these different data-driven techniques.

Presentation quality. The organization of the manuscript is good, and it only requires

C5080

some minor improvements (please see my detailed comments). On the other hand, there is a number of technical aspects that surely deserves more attention. For example, the description of the data-driven models (line 14, page 11091 – line 20, page 11092) is too synthetic and thus prevents the reader from understanding the experimental set-up (e.g., Table 2) as well as some of the results reported in Section 3. The introduction should also be strengthened – there is a number of statements that requires clarifications and additional references.

Scientific quality. The following aspects should be taken into account:

• The rationale behind the adoption of two different modelling approaches is not described clearly. Why are two different modelling approaches needed? Why do they lead to different results? • I have some doubts regarding the second formulation – Eq. (2)–(3). Streamflow anomalies are calculated by (a) subtracting the long-term average streamflow and (b) dividing this number by the long-term standard deviation. However, the streamflow process appears to be non-stationary – the changes in land use have an impact on the rainfall-runoff process, while the long-term average and standard deviation are calculated on the hypothesis of a stationary process. I think that the authors should elaborate on this point. • The experimental set-up is described only partially – some of the adopted techniques require more parameters than those listed in Table 2. This limits the reproducibility of the study. • I understand that NSE must be adopted to compare the results of this study against those obtained with physical models. Yet, if the authors acknowledge (and stress) the limitations of NSE, I do not understand why they have not used an additional (and better) metric, such as KGE. • Why does the climatology model perform so well? Given the results reported in Table 3, one might conclude that complex data-driven models are not needed since a simple climatological model can get excellent values of NSE and MAE.

I believe that this study may be of potential interest to the readership of this journal, but the manuscript should be reconsidered for publication only after a major review.

C5081

Further details and comments are outlined below.

Specific comments

- The title does not fully represent the content of the paper—in particular, water management issues are not explored in the study.
- Line 2, page 11084. Can you give an example of the “certain methods” mentioned here?
- Line 4, page 11084. “Data” should be used as the plural form of ‘datum’.
- Line 4-10, page, 11084. I do not completely agree with this statement. There is an extensive body of literature on the application of data-driven techniques to streamflow modelling problems—see, for example, Elshorbagy et al. (2010). Whilst model interpretability and uncertainty have received somewhat less attention, there have been studies focussing on such aspect—see, for example, Wilby et al. (2003) or Taormina and Chau (2015).
- Line 11, page 11084. I think the authors should explicitly mention the “machine learning” techniques used in their study.
- Line 20, page 11084. This sentence may be misleading, since the study does not carry out a climate impact assessment.
- Line 4, page 11085. Are the authors referring to Genetic Programming (Babovic et al., 2005)? Genetic algorithms are heuristic optimization techniques; as such, they cannot be directly employed for data-driven streamflow predictions.
- Line 24-27, page 11085. Yes, but this why there exists a variety of techniques—e.g., cross-validation, bootstrapping etc.—aimed at minimizing/reducing overfitting problems.
- Line 21-22, page 11086. Yes, this why different (or multiple) objective functions should be considered when training a model (De Vos and Rientjes, 2008).

C5082

- Line 1-3, page 11087. Again, I believe that during the past 5-10 years, several studies on data-driven streamflow forecasts not only focussed on improving model performance, but also on improving our understanding of the models structure—thus supporting the interpretation of the underlying physical processes.
- Line 7-9, page 11087. Can the authors expand the literature review on the use of data-driven techniques on non-temperate regions?
- Line 16, page 11087. Can the authors provide more details on “relevant landscape change”?
- Line 1, page 11088. This section should be named ‘Data and Methods’.
- Line 20-21, page 11088. Can you give an example of these infrastructures?
- The first part of Section 2.2 is about data, not models. Why not splitting it into two sections focussing on data and models, respectively?
- Line 22, page 11089. “monthly daily average temperature”?
- Line 28, page 11089. It should be “these data”.
- Line 4, page 11090. Does the land cover vary on an annual basis?
- Line 15-16, page 11090. This comment is about model results, not data. It should be moved to the results section.
- At the end of the first paragraph (Section 2.2) authors should clearly state what the number of available observations (for each catchment) is. Moreover, do the authors have an estimate of the time of concentration (of each catchment)? This relates to the time lags adopted for the precipitation in model (1) and (2)-(3).
- Line 27-28, page 11090. Do the streamflow data follow a log-normal distribution?
- Line 13, page 11091. It should be “Six”, not “Seven”.
- Line 6, page 11093. It should be Table 2, not Table 1. This error is repeated through-

C5083

out the manuscript.

- Line 18, page 11093. What is the reason for adopting the MAE?

- Line 20-22. Why?

- Line 19, page 11095. What is the "delta-change method"? I think that a short explanation is needed.

- Line 25-28, page 11097. One of the most common mechanisms for understanding the importance and influence of covariates is input variable selection—see, Wu et al. (2014) and Galelli et al. (2014).

- Line 27-28, page 11099. Is there any reason behind this?

- Line 6-7, page 11104. This comment is not necessary.

References

Babovic, Vladan, and Maarten Keijzer. "Rainfall–Runoff Modeling Based on Genetic Programming." *Encyclopedia of Hydrological Sciences* (2005).

De Vos, N. J., and T. H. M. Rientjes. "Multiobjective training of artificial neural networks for rainfall–runoff modeling." *Water resources research* 44.8 (2008).

Elshorbagy, A., et al. "Experimental investigation of the predictive capabilities of data driven modeling techniques in hydrology-Part 1: Concepts and methodology." *Hydrology and Earth System Sciences* 14.10 (2010): 1931-1941.

Galelli, Stefano, et al. "An evaluation framework for input variable selection algorithms for environmental data-driven models." *Environmental Modelling & Software* 62 (2014): 33-51.

Taormina, Riccardo, and Kwok-Wing Chau. "ANN-based interval forecasting of stream-flow discharges using the LUBE method and MOFIPS." *Engineering Applications of Artificial Intelligence* 45 (2015): 429-440.

C5084

Wilby, R. L., R. J. Abrahart, and C. W. Dawson. "Detection of conceptual model rainfall–runoff processes inside an artificial neural network." *Hydrological Sciences Journal* 48.2 (2003): 163-181.

Wu, Wenyan, Graeme C. Dandy, and Holger R. Maier. "Protocol for developing ANN models and its application to the assessment of the quality of the ANN model development process in drinking water quality modelling." *Environmental Modelling & Software* 54 (2014): 108-127.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11083, 2015.

C5085