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Hydrology and Earth System Sciences Discussions

Interactive comment on "Reservoir computing as an alternative to traditional artificial neural networks in rainfall-runoff modelling" *by* N. J. de Vos

Anonymous Referee #2

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This paper, which promotes echo state networks, provides an interesting comparison of standard and novel means of implementing neural networks for streamflow forecasting. It is well written, easy to read, and informative. Suggestions for improvement follow.

Major suggestions

The author is well aware that, despite their well-documented performance, NN are seldom used in an operational context because of their lack of interpretability (page 6102, line 26). This issue needs to be addressed further in the manuscript because even if echo state networks may surpass feedforward ones, it is unclear in what respect

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they would be better received by operational hydrologists.

I credit the author for using data from twelve watersheds, but was disappointed by his decision to limit the study to a one-day-ahead comparison. Multistep ahead comparisons are now imposing themselves as the norm in streamflow forecasting (e.g. Toth and Brath 2007; Yonoba, 2010). The author should consider extending his work.

It is quite difficult for a reader to compare performance in Figures 4 and 5. The author should provide a summary table of the best results or write values in the figures above (under) the boxes. He should also provide more information on MSEp20 and MSDE, which are nonstandard. It is merely impossible for the reader to assess if results illustrated in Figures 6 and 7 are good or not, and if the difference between models is significant. I know that modellers are always pushing for the best possible score, but at some points gains may be marginal and of no practical interest. This latter issue needs to be addressed in details.

I do not understand the link made between the optimal number of layers found for the LESN and the fact that FF functions well with a single layer (page 6117, line 28). Sections 4.2 and 4.3 include many methodological issues that should have been presented in section 2.3 for clarity.

Other suggestions

What is a biologically plausible NN and why is it important for streamflow forecasting (page 6110, line 12)?

The author needs to justify the use of a principal component preprocessing of the data (page 6111, line 18) – an unnecessary step from my point of view.

The author should specify the number of FF runs that was performed, in order to better assess results.

The author may use Yonoba et al. (2010) to justify his FF architecture (page 6113, line 20).

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Interactive Comment

Please remove the first or second “because” at line 12, page 6115.

Explanations for Figure 1 are not given with enough details, especially the error arrows.

Figure 8 should be included in Figure 1.

References

Toth, E., Brath, A. 2007. Multistep ahead streamflow forecasting: Role of calibration data in conceptual and neural network modeling. Water Resources Research, 43 (11), W11405.

Yonaba, H., Anctil, F., Fortin, V. 2010. Comparing sigmoid transfer function for neural network multistep ahead streamflow forecasting. Journal of Hydrologic Engineering, 15, 275-283.

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