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Interactive Comment

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

N.J. de Vos

njdevos@gmail.com

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I am grateful for your insightful and constructive review. I have taken care to implement your suggestions into the revised manuscript version.

Below a point-by-point response to your comments.

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 *they would be better received by operational hydrologists.*

In the revised manuscript, I have further elaborated on the similarity between recurrent networks and hydrological systems, and the potential benefits that this might yield. Regarding echo state networks, I have more strongly highlighted the specific advantages of their workings, including their easy-to-understand methodology and insightfulness (e.g. output is a linear combination of internal states). Although they are unlikely to fully replace current forecasting techniques anytime soon, I believe ESNs could be useful as members of a model committee, or in hybrid conceptual/empirical operational models. Nevertheless, echo state networks are still a fundamentally empirical approach, with all the pros and cons that come with the territory.

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; Yonaba, 2010). The author should consider extending his work.

I now present a figure with performances for 1 to 4 days ahead for the key models. The current data set mostly contains catchments whose residence times are about 1 day, though, and the differences between models are therefore often small compared to the overall deterioration of model results.

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.

First of all, the differences in scaling in Figure 4 has been largely resolved. There is a trade-off between uniformity of the scaling on the one hand, and precision due to the large differences between the catchments on the other hand. I therefore have opted for a trade-off by having only 2 different scales (CE from 0 to 1 or from 0.5 to 1), depending on the overall performance of all models on a catchment. (A note has been made in the caption to warn the reader of this.)

Secondly, I have used the CE of log-transformed streamflows as a low-flow indicator instead of the MSEp20. I believe that this measure is a better choice, especially in light of your suggested need for easier interpretability of the results.

Thirdly, the explanation of the MSDE has been somewhat expanded. It is not a normalized statistic, so direct comparison between catchments is difficult (comparison with the reference models (PM and LIN) for each catchment is more meaningful). The scaling of the y-axis has also been made more uniform.

Finally, I would like to point out that even if the current ESN models offer no significant performance increase, its competitiveness while being a conceptually simpler ANN model might offer benefits in future research (also note that research on ESNs is still in its infancy). I have elaborated on this point in the revised manuscript.

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).

In hindsight, the suggestion of any link is rather speculative. I have removed this statement.

Sections 4.2 and 4.3 include many methodological issues that should have been presented in section 2.3 for clarity.

These sections have been incorporated into sections 2 and 3, as suggested.

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

I have deleted this statement, since it refers to specific reservoir computing fundamentals that are of little interest and consequence to practical (hydrological) applications.

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.

After testing, I found that there doesn't seem to be any added value to the use of the PCA, nor have I found any convincing evidence in the literature for this. I have therefore no longer used it for input pre-processing.

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

I have updated the text and figure captions to clarify that the same number of runs (20) was used for all models.

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

Thank you, the reference has been added to the FF architecture discussion. Nevertheless, I have switched to hyperbolic tangent functions in all neurons, for the sake of a fair comparison with the Williams-Zipser network.

Explanations for Figure 1 are not given with enough details, especially the error arrows. Figure 8 should be included in Figure 1.

Figure 1 has been completely revised. Figure 8 has been incorporated in it, as suggested.

Final note: A correction has been made to the original manuscript: I now present results for both the original Williams-Zipser fully recurrent ANN, and a variation on this network where there are extra no-delay connections between hidden neurons and the output neuron. (This allowed for stronger direct non-linear capabilities compared to the original form of the network.) In the original manuscript, I mistakenly presented results

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for the latter under the name of the traditional Williams-Zipser network.

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