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Interactive Comment

Interactive comment on "Classification of heterogeneous precipitation fields for the assessment and possible improvement of lumped neural network models for streamflow forecasts" *by* N. Lauzon et al.

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Reviewer comments

Classification of heterogeneous precipitation fields for the assessment and possible improvement of global neural network models for streamflow forecasts Author(s): N. Lauzon, F. Anctil, and C. Baxter

General comments

The paper describes a double application of artificial neural networks, which are used first in unsupervised mode, for the classification of the precipitation fields characterising a case study basin and secondly in supervised mode, as a systemic rainfall-runoff model. The idea of distinguishing the precipitation events that are provided as input to the rainfall-runoff model is very interesting and the classification by Kohonen networks is certainly an adequate tool, as the good results confirm. In the second application (which may nowadays be considered "classical", due to the large number of applications in the field) the performances of the rainfall-runoff simulation for the different classes are examined. A part from the fact that I do not completely agree with the given interpretation of the results, I think that the procedure for the choice of the input data (different spatial representations of the precipitation data) should have been based on the results of the classification phase or following a more objective method, rather than subjectively. More importantly, a larger advantage from the above obtained classification would have been gained from the use of separate ANN models for the different classes, as underlined also by the authors. It is therefore certainly a good and original work but it may be more complete.

Specific comments

Scientific methods and assumptions are valid and clearly outlined; title and abstract reflect and adequately summarise the contents of the paper and the presentation is well organised, clear and concise.

As above said, the choice of the input data (different spatial representations of the precipitation data) is not exactly and objectively based on the results of the classification phase, as it would seem from section 3.2 (p. 210, ll. 14-15), but it is done subjectively and I would suggest to clarify it from the beginning (p. 210) and also in section 4.2 (p. 213, ll. 14-19).

The description of the choice of the architecture is not clear (section 4.2, p. 213): in table 2 the number of hidden nodes is different from 5 but its optimisation does not follow any input selection phase.

Section 4.2 and Table 3: I do not agree with a comparison between the SSE obtained for different classes: SSE is not even a mean value (eq. 3) and it cannot be compared for different data sets. I would not compare RMSE for different groups, either, because, even if at least it is a mean value, its comparison is significant for different methodologies applied on the same data set but not for different data, since such measure is certainly (as acknowledged in the following also by the authors) dependent on the streamflow values, due to the heteroschedasticity of the series: larger errors are expected for larger streamflow; it follows that it is obvious that RMSE (and even more the SSE) is much higher for group 3, which corresponds to the highest precipitation and streamflow values. Therefore I do not agree with the conclusion that the model can not take into account the heterogeneity, because the reason of the larger SSE and RMSE of group 3 is not the heterogeneity but the high streamflow values. This is partly confirmed by the good values obtained for group 3 for the persistence index, which is one of the best and most exacting goodness-of-fit measures and it is certainly much less sensitive to the effect of largest errors for largest streamflows, being divided by the naive error. I would therefore suggest to revise the interpretations presented in section 4.2 (p. 214, ll. 6 to 29) and in the Conclusions (p. 216, ll. 13-16).

Pag. 215, ll. 4-7: I believe that it would be useful to highlight that the main problem evidenced in the applications with spatially distributed input (rather than relating the worsening to the highest SSE, for the considerations above made) is that it causes a deterioration of the results especially for the more heterogeneous precipitation fields, that is groups 1 and 3 in the 3-groups classification and groups 1,3,4 and 6 for the 6-gropus classification, whereas for such cases it would be expected the major advantage of a spatially distributed input.

Pag. 215, ll. 8-10: as said in the general comments, I agree with the authors on the necessity, for a more rigorous and scientific approach, of testing a more objective technique for the gauges combinations, but I suspect that the importance of parsimony (and therefore of a small number of input nodes) will always be dominant in comparison with the advantages provided by a more detailed precipitation description. An application with a more objective procedure would anyway be useful for testing such hypothesis.

Pag. 215, ll. 11-16: as said in the general comments, I would suggest to fully implement and show the results of the tests using different networks for different classes because it indeed seems a promising approach and such application would be the best completion of the proposed classification method, making it an important tool also for practical purposes. I would think that a minimum of 569 training samples (the minimum size of the 6-group classes) should be enough for the identification of a parsimonious network for each class.

Pag. 215, ll. 17-19: maybe because of the different interpretation I give to the results, I do not understand the meaning of this paragraph (and also of the corresponding one in the conclusions, ll. 20-22).

Technical corrections

All the figure numbers must be corrected (evidently there were two additional figures in a first version of the manuscript and they are all shifted).

Pag. 212, l. 7: I would say that the majority but not “almost all the events” in groups 3 and 4 of the 6-group classification are located in groups 1 and 3 of the 3-groups classification.

To help the reader to follow the text, I would suggest naming the classes (groups) with letters (the same identifying the figures) instead of numbers and a number for the classification type, e.g. 3a, 3b, 3c for the 3-group classification and 6a to 6f for the 6-group classification.

Table 3: please specify the networks architecture (I guess there is the same number of hidden nodes of table 2, but it would be preferable to repeat it since in the text, p. 213, there is a reference to an optimisation following an input selection phase). The results for group 4 (in the 6-groups classification) are exactly the same with one or two precipitation inputs: are the data exactly the same?

Fig. 2, Fig. 3 and Fig.5: please specify in the captions the meaning of the isohyets lines on the maps.

Fig. 4.: these figures are not clear to me: in Fig. 4a are the distributions of the mean values obtained for the different gauges, that is the distributions (one line for each group) of the samples of 23 values reported in Fig. 3a to 3f? How are they obtained?

Interactive comment on Hydrology and Earth System Sciences Discussions, 3, 201, 2006.