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Hydrology and Earth System Sciences Discussions

Interactive comment on "Towards model evaluation and identification using Self-Organizing Maps" *by* M. Herbst and M. C. Casper

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General comments

The paper describes an application of unsupervised neural networks (Self-Organizing Maps SOM) for the classification of the output of a distributed rainfall-runoff model, that is for the classification of the model simulation series. The idea of distinguishing the simulation series through an unsupervised network, rather than by goodness-of-fit indexes that necessarily aggregate the information on the simulation errors (thus loosing a part of the information on the model performance), is very interesting and, to my

knowledge, absolutely novel in the hydrological literature. The classification by SOM networks is certainly an adequate tool, as the good results obtained by the authors confirm. Title and abstract reflect and adequately summarise the contents of the paper, the presentation is well organised, clear and concise, the references are relevant and up-to-date, tables and figures are adequate and all necessary.

The only relevant reservation I have about the work is the use of the technique for optimisation purposes, that is for identifying one or more optimal parameter sets. In fact, the proposed technique allows to better evaluate the model simulations (better than the computation of one or more classical error measures) but it does not guide the search of the parameter space for identifying the best parameter vectors, like a real optimisation algorithm does. The analysis of the simulations issued by the Monte-Carlo approach (which by the way should be described with more detail) does not guarantee that the parameter sets that are tested are the best performing and even if the number of simulations is very high for a distributed model, the possible parameter sets are obviously much more numerous. Considering this aspect, the comparison with the SCE-UA optimisation algorithm does not fully convince me: a) the Monte-Carlo simulations shown in Figure 5 are all good, probably too good and this may be due to the choice of the feasible parameter space, which was guided by the manual expert calibration (and this is not the way automatic calibrations are implemented and may unfairly facilitate the work of the) b) the results obtained by the SCE-UA algorithm do not minimise the RMSE (see Table 4) and this is certainly peculiar, since the algorithm is conceived just to minimise this measure and it should at least get to the lowest (or a comparable) RMSE values obtainable with the set of Monte-Carlo simulations. More details on the SCE-UA optimisation must be added and the authors should also consider to re-run it (maybe it was trapped in a local minimum?), given the unsatisfactory results.

The work is certainly valuable, the proposed technique is original and may give precious insights for evaluating the simulations of the rainfall-runoff models, but, whereas

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its usefulness is clear for identifying the behavioural parameter sets in a (already identified) larger set within an equifinality framework (for example in a GLUE approach), I do not believe it can be used, at the moment, for optimisation purposes.

Specific comments

p. 2, l. 1: the authors may specify which "considerable problems" are involved in the use of the measures.

p. 3, penultimate paragraph: as above said, an optimisation algorithm does not only identify the good model realizations (goodness-of-fit measures are sufficient for this purpose) but it guides the search of the parameter space.

p. 7, penultimate paragraph: as above said, the aspect of the identification of the parameter bounds on the basis of the manual calibration should be clarified.

p. 8, first line of section 3.1: is there a reason for choosing a 22x15 grid?

p. 9, 4th line of section 3.2: Eq (8)?

p. 9, ll. 8-9 of section 3.2: how did you find the combined optimum region?

p. 10, ll. 8-9: as above said, more details on the SCE-UA implementation should be added. Furthermore, rather than, or in addition to, the zoom of the hydrographs of Fig. 5, scatterplots may be added, that are more representative of the entire model simulations.

The meaning of the last paragraph of section 3.2 ("It is interesting to note...") is not clear to me.

p. 11: first section: the interpretation of Fig. 2 as far as RetBasis and hL are concerned should be first reported in section 3.1 and then recalled in the conclusions.

Technical suggestions

p. 7, l. 12-13: I would rearrange the phrase like this: "Consecutively, the reference

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data set consisting of the time series of observed data, which has not been part of the training data, was projected..." p. 9, l. 10 of section 3.2: I would suggest to add "...that are associated to the SAME BMU..."

(P.S. I know self-citation is not orthodox, but I would like to signal to the authors that also the paper: Montanari A., E. Toth (2007), Calibration of hydrological models in the spectral domain: An opportunity for scarcely gauged basins?, Water Resour. Res., 43, W05434, doi:10.1029/2006WR005184. implements a calibration in the frequency domain like the other two papers cited at p. 2)

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