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Hydrology and Earth System Sciences Discussions

Interactive comment on "Soft combination of local models in a multi-objective framework" *by* F. Fenicia et al.

Anonymous Referee #2

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In this paper the authors propose a modular strategy to improve predictability of environmental systems. The method starts out with defining different parts of observed system response and calibrating the same model structure to these different events using multiobjective optimization. This results in an ensemble of models with different values for the optimized parameters to reflect different portions of the hydrologic behavior. A fuzzy weighting scheme is then used to combine the forecasts of these individual models. So, instead of using multiple models simultaneously to cover the entire range of system response, the authors propose a modular strategy to hydrologic forecasting. They demonstrate the utility of this approach by application to streamflow forecasting using discharge data from the Alzette River basin in Luxembourg.

The paper presents an interesting idea, but needs sufficient more attention before it

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can be judged to making a significant contribution to the water resources literature.

Major comments:

1. I think that the presented method is not that novel. For instance, how does the present method compare with the Self Organizing Linear Output Map, published by Hsu et al.:

Hsu K., H. V. Gupta, X. Gao, S. Sorooshian, and B. Imam, Self-organizing linear output (SOLO): An artificial neural network suitable for hydrologic modeling and analysis, Water Resour. Res., 38 (12), 1302, doi:10.1029/2001WR000795, 2002.

This approach seems to be much more advanced than the work presented in the current paper. In SOLO, the authors use a Self Organizing Map (SOM) for system classification and to cluster different parts of system behavior. In each of these clusters a linear model is calibrated, and used for streamflow forecasting. Although, no implicit weighting scheme is used when forecasting, this method is completely automated, and provides useful information about the complexity and nonlinearity of the watershed (interpretation of the SOM nodes). The proposed procedure is far from being "objective" in its current implementation and discussion. For instance, are there any guidelines how many models to use and what objective functions to implement in the proposed procedure. Also how does the weighting procedure affect the forecast error? This is essential and not discussed in the paper. Ultimately, these decisions are going to determine the success of the procedure.

2. The paper would significantly improve in quality if the authors compare the results of their proposed modular strategy in terms of forecast error and variance with other multi-model approaches already out there. Specifically, I am referring to work by Shamselding et al. (1997), Xiong et al. (2001) using different model combination methods, or the recent work by Ajami et al (2007) on Bayesian model averaging and Vrugt et al. (2007) on comparison of BMA and Ensemble Kalman Filtering for streamflow forecasting:

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Ajami, Newsha K.; Duan, Qingyun; Sorooshian, Soroosh, An integrated hydrologic Bayesian multimodel combination framework: Confronting input, parameter, and model structural uncertainty in hydrologic prediction, Water Resour. Res., Vol. 43, No. 1, W01403 10.1029/2005WR004745

Vrugt, Jasper A.; Robinson, Bruce A. Treatment of uncertainty using ensemble methods: Comparison of sequential data assimilation and Bayesian model averaging, Water Resour. Res., Vol. 43, No. 1, W01411 10.1029/2005WR004838

From the current paper is it not clear at all what the advantages are of the proposed procedure. Does the method result in improved forecasting capability or hydrologic understanding? If not, this is just a stand-alone paper, without making clear what the advantages are of the proposed approach.

Minor comments:

Page 93 - Second paragraph: Another disadvantage of making models more complex is that there is usually no independent information to test the validity of this additional complexity.

Page 93 - Third paragraph: Define what you consider to be a different model: Different model structures, or the same model structure with different values for the parameters?

Page 110 - around 20: What about errors in the input (rainfall, PET)? This is probably more important than errors in model structure.

Figure 6 & 7: x- and y-axis: These values for the objective functions are very difficult to interpret. Is it possible to use a RMSE or so by rearrangement of the objective functions?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 91, 2007.