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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.

F. Fenicia et al.

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Answer to anonymous referee 2

The authors would like to thank the referee for his useful comments.

Major comments: Referee: 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).

Authors: The referred paper presents a method of decomposing the data (using SOM) and building local data-driven (linear regression) models for each of the data groups. Indeed this is an interesting method, but this method is purely data-driven since it does not use the knowledge about the physics of the modeled phenomenon and is based on linear regression models. Our approach is completely different and it is a pity that the referee has not appreciated the differences. Firstly, we use conceptual models, and not data-driven ones. Secondly, we do not use clustering (data-driven) procedure to identify the regions in the state space to build local models, but rely on the physically-based analysis (explicitly distinguishing low and high flows). Thirdly, we combine local conceptual models using physical considerations again, and the SOLO method switches the local models on and off, whereas our approach ensure smooth combination of models. Fourthly, we applied multi-objective calibration. We feel this makes SOLO and our approach quite different indeed. However, we appreciate the mentioned paper and the reference to it is now included in the revised paper.

Referee: 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.

Authors: We do not claim our approach is "objective" - it has enough empirical features and subjectivity which can be subject to critical analysis and criticism. They are clearly explained in Section 2 of the paper. Indeed we do not provide guidelines on how many models or what objective functions (OF) to use. It would be interesting to investigate the usefulness of having more models, but this was not our main objective (and can be done in the future). The types of OF can be different (a valid point!), and Section 2 contains some of our deliberations on that. However, this was not in the scope of the paper

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in which we wanted to show the usefulness of a composite modeling approach. The possible enhancements to this work are stated in the new section “Future research” in the revised paper.

Referee: 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.

Authors: The referee is absolutely right that the “these decisions are going to determine the success of the procedure”. We think, in terms of analyzing how “the weighting procedure affects the forecast error” we do quite a lot. Our approach is to perform exhaustive optimization of the weighting scheme - by finding optimal values of gamma and delta. Indeed, the weighting function can be different and its influence on the results can be investigated; this is planned to be done in the future research, and mentioned now in the final Section of the revised paper.

Referee: 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.

Authors: We thank the referee for pointing at the mentioned 4 papers. Note however, that two of them were already referenced, and the other two were published after the paper was submitted. Indeed, comparison to other combining methods would be a very useful thing to do. However it was not an intention of our paper where we compared the new combinations method to the global (overall) HBV model traditionally used in catchment modeling. Unfortunately, the size of the paper did not allow doing more. However, in a qualitative way, we will comment on the difference between our approach and others in the discussion section of the revised paper.

Referee: 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.

Authors: We are a bit surprised that these questions are posed. Our understanding is that we clearly demonstrated that the proposed approach results in the better performance (lower error). (Term “forecasting” used by the referee we would use with care since HBV is a simulation model - which can however be used in forecasting mode). Another matter is: have we achieved a better hydrologic understanding? We think we did, at least partly - and discussion on this can be found in Section 4, following the sentence “While the practical utility of such an approach relies on an improvement of the simulation accuracy, the physical implications involved require interpretation and justification”. In the revised paper, we updated this part, making it clearer. Note that in the revised paper terminology was slightly changed to make it consistent with our other papers on modular and composite modeling: the overall HBV model is now called “global model”, and the combination of local HBV models is called “composite model”.

Minor comments: Referee: 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.

Authors: True, we will add this useful comment in our revised paper.

Referee: 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?

Authors: We will clarify this point.

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

Authors: They are very important as well. We mention this point Pag. 101 (8-19), but we will make it clearer.

Referee: 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?

Authors: As mentioned in the answer to the other referee, we will add a table or graph indicating model performance with respect to other types of objective functions also.

Authors: We would like to thank referee 2 for the time spent on reading and commenting the paper, and the useful suggestions.

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

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