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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparison of two model approaches in the Zambezi river basin with regard to model reliability and identifiability" *by* H. C. Winsemius et al.

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While a significant progress is achieved in hydrologic modeling, its practical application at the watershed scale still requires tremendous efforts and subjective judgments on the user side. There is a wide range of available and recently developing hydrological models of different complexity and usually not well defined applicability. Most models are tested and evaluated locally, even if applied for different regions, without producing of information on a dependency of model parameters and physical properties/climate to generate their spatial patterns. Model application at a desired location becomes a highly 'empirical' process that significantly relays on calibration results even if required data are limited. The paper of Winsemus et al. deals with such a problem for the

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Discussion Paper

Zambezi river basin where data sources are very limited and aggregated over large areas.

The paper analyzes results of application of two hydrological models over a few catchments of the Zambezi river basin: a distributed grid based (STREAM) and lumped (LEW) models. The both models are storage based, conceptual. While they have a similar physics, a spatial interaction of grid cells (in STREAM) or hydrological units (in LEW) differs significantly: there is no any spatial interaction in STREAM, but LEW units can interact to redistribute runoff generated in neighbor units. It is not clear why STREAM was selected to model this basin if the authors stated that spatial interaction should be accounted to represent this basin hydrology well. It is also not clear what time scale was used in applications. One statement suggests that 'Two different model structures were developed, at monthly scale Ě'. However, later on they mentioned daily scale 'Ě evaporation within the same day the rainfall took place'. Were models run at the daily time step, but all comparisons and statistics were calculated at the monthly time interval? This is critical point in further analysis. The authors used GLUE in analysis of parameter sensitivity/reasonability, and in selection of model configuration for the specific catchments. Considering very limited and noisy information as well as poor representation of the basin hydrology in STREAM, the authors come to an expected conclusion that the LEW structure looks better than STREAM. The paper confirms one more time that reasonable practical results can be achieved using a simple semi-distributed model if significant expert knowledge and subjective judgment is applied during selection of basin configuration and its parameterization. I do not think that this analysis allows a conclusive judgment on representation of physical processes by both models if just monthly simulation estimates are evaluated. Unfortunately, GRACE is mentioned as true 'orthogonal' information but these data were never used in the paper to refine models and constrain their parameters. That is why conclusion on a better representation of the interannual storage variation in LEW can not be drawn from this qualitative analysis.

The authors achieved a practical result by applying their knowledge on the basin specifics. Results suggest that just a model itself does not guarantee reliable results without well defined parameterization procedure. Unfortunately, proclaimed GRACE information was never used which restricted analysis and reduced the scientific outcome. Selection of the STREAM model is questionable because one can expect it will fail for such basin. The paper will be a valuable contribution to the watershed modeling problem as an example of an expert type parameter estimation process. Some general considerations/discussion could be drawn based on this and many other publications on watershed modeling:

1. Selection of the model structure. Is it a reasonable assumption that the model structure should be selected/adjusted to available limited data by using a calibration process that is very sensitive to data errors and to selected calibration criteria. I agree with the authors' hope to improve this process by using 'orthogonal' information, e.g. GRACE. However, how much and what kind of quality is enough to get not only satisfactory simulation results of a few variables but also represent a 'true' physics to obtain satisfactory simulations for ungaged, uncalibrated basins or climate change scenarios. While for many practical applications for specific goals/basins such approach may be reasonable, it is not scientifically sound. Actually, the authors do not select the model structure, but a semi-distributed basin configuration.

2. Parameter identifiability. Is GLUE one-dimensional analysis a true test of parameter identifiability. It is true that the more dependent model parameters are the more difficult is to achieve reliable solution by automatic calibration using limited noisy data. However, parameters are physically dependent, and a calibration process should account for these relationships. The authors of the referred paper mention constrained calibration by using additional 'orthogonal' data. It seems to me that less attention in the hydrological community was paid to a better definition of a priori parameter sets by establishing 'soft' relationships between model parameters themselves, and physical properties and model parameters. Filtering model physics by using an unconstrained

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Interactive Comment

automatic calibration on noisy input/output data may sometimes provide a practical result but can not guarantee it in the predictive mode specifically if data errors are invariant. Effect of 'less calibration' and a priori parameter uncertainties could An ensemble approach could reduce an effect of 'lesser calibration' when a priori estimates would be used for the most parameters instead of pure calibration.

Some minor comments on the paper: 1. 'Orthogonal' information does not sound well. In addition, one can expect a significant correlation between discharge and terrestrial water storage. 2. Clarify the modeling time step. Do the models run at the monthly time step or daily? 3. Move the paragraph starting from 'Su,max was defined .. ' on page 2636 before the statement 'The saturated zone Ě'. 4. Add some explanation on the selection of the STREAM model for the analysis. 5. I do not agree with the statement 'In the Zambezi basin, threshold behavior is the main cause of non-linearity.' It seems to me this is conceptualization of physics not true threshold physics. Actually, in space there are distributions of different compartment storages which control a transition from one state to another, but not a jump.

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