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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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GENERAL COMMENT The paper presents a comparison between two conceptual hydrological models - a grid operated distributed model (STREAM) and a sub-watershed semi-distributed model (LEW). The two models have been applied to the upper Zambezi river basin at monthly time step. The paper examines the identifibility of the model parameters of the two models and the model structure of the simpler semi-distributed LEW model using the GLUE method. The results show that a good knowledge of the physical processes of runoff generation could help the hydrologist built a simple and representative model. The paper is, in general, well organized and written but there are a few points that should be clarified and addressed. I think that addressing these points

will help the reader and strengthen the paper. Overall, the paper merits publication in the HESS after the comments are properly addressed.

MAJOR SPECIFIC COMMENTS The major specific comments are: Firstly, a better and self-explanatory map of the study area should be given. This map should contain the sub-basins of the upper Zambezi basin, the flow gauging stations used, the country boundaries and the tributaries of Zambezi river. In this sense, such a map will replace Figure 1 of the paper and help the international reader of the paper to understand the description of the study area. Secondly, the authors present and discuss the Gravity Recovery And Climate Experiment (GRACE) data set in many parts of the paper starting from the Introduction section. However, the GRACE data set was not used in the analysis presented in the paper. It is understood that this data set will be used in future analysis. I suggest that the abilities and the opportunities given by the use of GRACE data in hydrological modeling should be discussed at the end of the paper showing the direction of the future study. Also, I suggest that the authors rewrite the Abstract of the paper in order to be consistent with the previous point and the scope of the paper. Thirdly, my main criticism on the paper is about the calibration-application of the two models. The points of concern are: 1) It is not clear how many are the optimized-calibrated parameters of the two models. I infer that four parameters of the STREAM model have been optimized (D, qc, cmax, cr). Also, it is not clear how the parameters Su,max (by the land use map) and Muskingum flow routing are estimated (pages 2634 and 2635). It is more unclear how many are the optimized parameters of the LEW model (Only Qmax?). Furthermore, Talbes 1 and 2 show the values of same of the model parameters for various sub-watersheds. It is not clear whether the authors have calibrated the models at these subwatersheds. I suggest that the authors clarify this issue by clearly giving the estimated spatially variable, the optimized, and the estimated spatially invariable (constant) parameters of the models. 2) It is not clear the spatial representation of the study basin in the two models. What grid size has been used for the STREAM simulation? Is it 3X3 km? How many sub-watersheds of the upper Zambezi river basin have been used for the LEW modelling? What methodology has

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been used for the interpolation of the meteorological data? The spatial representation of such a large basin is a critical question, especially in this area where data are limited. I suggest that the authors apply a much simpler spatially lumped model (for example LEW lumped model) and compare with the previous results. 3) It is not clear how the calibration has been performed. The authors state that the two models "Ĕ have been calibrated on discharges at Lukulu and Victoria Falls". Are the optimized-calibrated model parameters spatially invariable (i.e. the same parameter values for all grids and sub-watersheds) or spatially variable? If the optimized-calibrated model parameters are spatially invariable then the spatial variation of hydrological processes modelling is concentrated on the input meteorological variables and the spatially variable estimated parameters (if such parameters exist). This is a spatially distributed (in the case of STREAM) or semi-distributed (in the case of LEW) modelling approach with lumped parameters. On the other hand, if the optimized-calibrated model parameters are spatially variable, then, their number is quite large and the question of equifinality is more evident. 4) It seems that the authors use the whole available time series of meteorological and discharge data for calibration. A better testing of the model performance is the application of the "split sample test" by keeping some data for the validation and testing of the model calibration. I suggest that the authors use this procedure. Fourthly, the discussion on the representation of the storage variation of the two models is qualitative and subjective since no measurements have been used to compare, at this point. It remains to test and validate the results for the storage variation of the two models with the use of GRACE data. However, no definite conclusions could be drawn at this paper.

MINOR TECHNICAL COMMENTS

1. Page 2626 line 28. The use of term "orthogonal" is not appropriate here. This is a mathematical term. 2. Page 2631 line 8. Ĕ.1300. Add units (mm/yr). 3. Page 2634. Is D constant irrespective of rainfall, temperature, evaporation? 4. Page 2639 line 9. Correct "mereits" to merit. 5. Title of Table 1. Clarify what it is meant with the term

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spatially variable.

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