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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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This article presents a comparison between two models (namely LEW and STREAM) to represent the hydrology of the upper Zambezi river watershed at the monthly time-scale. The article is presented as a starting point for further research on the assimilation of Gravity Recovery And Climate Experiment (GRACE) data within these two models, in order to constrain the models parameterization and/or to improve models efficiency. The efforts made to collect hydrological data in sufficient quality and quantity are praiseworthy. The article is well written, concise and easy to understand.

The main issues raised by the authors are: 1. Which spatial representation is needed to model a large basin such as the upper Zambezi river basin at Victoria Falls? This is a critical issue for this very large basin, particularly in the context of both low availability of historical data and potential future additional spatial data. 2. Which scientific tools allow discriminating hydrological models for specific applications? This issue is dealt with along the paper with respect to the up-to-date scientific tools. Thus, the paper presents interesting methodological guidelines in order to select and diagnose hydrological models. Thus, this paper has both scientific and operational reaches.

My main critics concern the initial selection of the two hydrological models used and the discussion on the potentialities to use GRACE data to increase model performance and model parameter identifiability. I would like the authors concentrate more on the ability of the models to simulate discharges, with regard to their spatial representation of the basin.

Specifically, I would like the authors discuss/comment the four following points.

1. Scope of the paper. I found the introduction and the abstract misleading. It is stated that “The goal of the modelling exercise is to eventually compare modelled storage with GRACE observations”. Yet, such testing is only a perspective that is not dealt with within the paper. To avoid confusion, I suggest that the authors put the introduction of the paper into another perspective, more in adequacy with the title of the paper. Indeed, this paper is an interesting comparison of two strongly different spatial representation of the watershed. GRACE opportunities should only be discussed at the end of the paper.

2. Comparison of tested models. The authors have chosen two strongly different modelling approaches (distributed and semi distributed) but that present large similarities in their conceptual structures, with regard to the myriad of existing hydrological models. To me, the essence of the comparison is the spatial representation of the basin rather than the structure of the model. An additional approach is missing in order to complete this comparison: the lumped approach over the entire upper Zambezi watershed at

Victoria Falls. This could be implemented (without many efforts and time) using the structure of the LEW approach. This third approach could allow the authors to discuss the spatial data requirements to simulate monthly discharges of a large basin such as the Upper Zambezi watershed, while analyzing the performance of the whole spectrum of spatial representations of hydrological models: distributed, semi-distributed and lumped. Given the area of the studied watershed, the lumped approach will probably lead relatively poor monthly discharge simulations, but the magnitude of the differences with the semi-distributed LEW approach could be very informative.

3. The structure of the models and time step of the study. I found both model structures quite complex for monthly discharge simulations, particularly the routing part of the models. The model structures appear to have been developed to represent daily discharges (and daily processes). However, if I well understand, only the monthly time step is used for the testing. Are the authors using a model developed at the daily time step for monthly simulations? In fact, the influence of the time step on model structure is considerable and I wonder if the structures used in the article are adapted to the monthly time step. In particular, some simplifications on the routing function would probably increase parameter identifiably without being detrimental to model performance. These doubts are supported by the results obtained with the (Generalized Likelihood Uncertainty Estimation) GLUE analysis (Figures 7 and 9): for both LEW and STREAM approaches, the routing parameter (respectively Sd and Ss,min) show a poor identifiability.

4. Representation of the interannual storage variation by the models and potentiality of GRACE data. The two conceptual models have been developed to simulate discharge at the outlet of the basin and they apparently do the job. However, if the results obtained by the two models can be considered good for discharge simulations, it does not mean that they could achieve good storage variation simulations. This is particularly well demonstrated by the authors through the comparison of the storage simulations, which shows large discrepancies according the model approach. Therefore, I disagree with

their belief that GRACE data could constrain the model parameters. If the authors wish a model that predict another output (like the storage variations), it is a new problem and the whole modelling development has to be resumed. This implies the definition of new objective functions and to address difficult issues such as the guidelines for dealing with Pareto optima. Anyway, I would like to share the optimism of the authors about the potentialities of GRACE data and I am eager to see their further research on this topic but no conclusions can be drawn at this point of their investigation.

Technical corrections

(1) p. 2639 Line 9 Typo: replace “mereits” by “merits” (2) I disagree with the statement on p. 2633 “It is assumed that it provides a coarse [potential evaporation] estimation, but this is expected to be appropriate because evaporation is mostly limited by soil moisture and not by the available energy”. If I have well understood the functioning of the models, potential evaporation PE also plays a critical role in determining the level of the soil moisture reservoir. Thus, a coarse PE estimation should affect the soil moisture reservoir level and thus also the evaporation estimation for the next months? I am not disturbed by this coarse PE estimation, but the justification given by the authors is not clear and should be reformulated. (3) p. 2634: is interception independent to PE, and only depend on rainfall and calibrated parameter D? It surprises me since in most hydrological models, “interception” is a function of rainfall and climatic variables.

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