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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.

H. C. Winsemius et al.

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We greatly appreciate the comments provided by referee Victor Koren. The referee has clearly taken the time and effort to read our manuscript thoroughly.

First of all, the referee addresses the fact that it is not clear why the LEW approach was not applied in the first place. The main reason why STREAM was chosen was because STREAM offers a framework to use spatially distributed information. A clear example of spatially distributed input data is rainfall, such as the FEWS RFE 2.0 product, described in the paper. Other important spatially distributed data include land cover, soil type and depth, topography and evaporation. Moreover, a spatially distributed (grid) model may improve simulation of peak discharges. This is due to the threshold behaviour of the model structures used. Storage thresholds may easily be exceeded locally, when

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spatial distributed rainfall is used. When the rainfall time series are spatially lumped, the peaks in rainfall will attenuate and result in less exceeding of thresholds. However, we discovered that the redistribution of surface runoff is a more important phenomenon and that runoff is also groundwater driven with a relatively long timescale. Furthermore, the 16 delineated watersheds in the LEW model provide enough distribution in view of the large scale characteristics of GRACE measurements. We will clarify this in the final paper.

Another comment is that the model time step is not clearly mentioned. The time step used is 1 month. The confusion is caused by the sentence on page 2634: “The fast evaporation consists of interception (evaporation within the same day the rainfall took place) Ě”. It may suggest that for computation of interception, a daily time step is required. However, the sentence refers to the physical period of time in which this process takes place. Usually, interception occurs directly after, or even during rainfall, meaning that intercepted water evaporates within the same day as it precipitates. In our model structure however, the evaporation of intercepted water is computed on a monthly basis.

Furthermore, the author of the comment disagrees with the statement that monthly simulation estimates do not provide enough analysis material to draw conclusions about which model better represents the physical processes occurring in the river basin. It is known that the time scales of processes in the upper Zambezi are usually long, meaning that the basin reacts slowly on occurring rainfall. This can be concluded from the recession coefficients K_s and K_q that are in each sub-catchment larger than 1 month. These long timescales can be explained from the low slopes and wetlands that are present in the upper Zambezi. Runoff in the Zambezi is primarily sub-surface runoff with a dominant contribution of groundwater (the dambo system, explained in the paper). We will explain this in the final manuscript.

Finally, the referee mentions that the orthogonal data from GRACE has not been used in this study, while these data may verify the interannual storage variation. The time

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series of GRACE storage models is at this moment simply not large enough to include these data in our parameter optimization procedure. However on page 2641, we mention that after a sequence of dry years it can take longer than expected before above average rainfall generates discharge at Victoria Falls. This is caused by low groundwater levels throughout the basin, especially in the floodplains upstream of Victoria Falls. After a sequence of dry years, these reservoir-like areas should be replenished by rainfall in the first wet month(s) before a reasonable discharge can be expected at Victoria Falls. Thus, although our conclusion about interannual storage variation is not drawn from quantitative information, we do have a strong indication that this phenomenon is indeed occurring in the upper Zambezi. Therefore we expect that the LEW model structure better represents the true physics. We will clarify this explanation in the manuscript.

Concerning discussion point 1, we agree with the referee that it is complicated to assess the quality of orthogonal data, needed to be able to refine a model and/or model structure. We do however feel that not only the quantitative value of data should be relied upon in model optimization but also the signature of time series should be analyzed and compared. In this way a data set that offers unreliable quantitative information can still offer information on the temporal pattern of processes. The question remains how patterns of resemblance should be recognized and quantified when used in an automated calibration procedure such as GLUE. The last remark that claims that the authors do not select the model structure but a semi-distributed basin configuration is probably a confusion of terminology. We use the term ‘model structure’ for the conceptual configuration of the hydrology such as is given in fig. 8 of the refereed paper. The delineation of watersheds, which the referee calls the semi-distributed basin configuration however, is purely physical, based on elevation data.

Discussion point 2 addresses the problem of parameter dependency. We agree with the author of the comment that a priori estimates of certain parameters can help identify valid parameter values / ranges for parameters, subject to calibration. We have

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applied this by fixing parameters that can be derived from the available data, in this case discharge measurements. K_s, K_q and $S_{d,th}$ were fixed in this way by analyzing the behaviour of the recession curve.

Concerning the 5 minor comments: 1. "Orthogonal information does not sound well. In addition, one can expect a significant correlation between discharge and terrestrial water storage." We have chosen the term 'orthogonal data', because it clarifies that data is completely independent from other available data. Therefore we would like to keep the term 'orthogonal' in the final paper. Furthermore, we do agree with the author of the comment that discharge is related to storage, however due to the non-linear behaviour of the watershed, the reaction on rainfall surplus of the river basin with respect to discharge may differ significantly with different states. Therefore we do consider storage observations to be orthogonal, independent information.

2. "Clarify the modelling time step. Do the models run at the monthly time step or daily?" The chosen time step is months. We will clarify this in the final paper.

3. "Move the paragraph starting from '$S_{u,max}$ was defined .. ' on page 2636 before the statement 'The saturated zone ..'" This will be altered in the final version.

4. "Add some explanation on the selection of the STREAM model for the analysis." As stated earlier, we will clarify the selection of the STREAM approach.

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." This is true, however, a spatial probability function of a threshold is also a threshold and behaves still explains non-linearity. The wetlands (dambos and floodplains) in the upper Zambezi are one of the major causes of this threshold behaviour. A dambo works like a reservoir. It fills up with water from rainfall and upstream flows. A stream, located downstream from such a wetland will only start flowing when it is replenished up to the

level of the streams river bed. Even when extreme rainfall occurs the dambo systems work like this because they consist of highly permeable Kalahari sand, which allows for very high infiltration rates. The Barotse floodplains can be considered as a huge dambo system.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2625, 2005.

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