

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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Rebuttal Loukas

We highly appreciate the comments of dr. A. Loukas. The points of comment are clear and we shall certainly address them in the final manuscript.

The first comment on the map we used is accepted. We shall clarify the geographic locations better by showing the country borders, main tributaries and the runoff stations used in the study.

The second point of discussion addresses the fact that we mention GRACE in the introduction and abstract. We do this, because we want to clarify beforehand what the goal of the modelling exercise is, namely introduce GRACE as a new source of in-

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formation to enhance hydrological models. Hence, the reader can judge if the model structures applied are suitable for this exercise. Therefore, we also show the discrepancies between the output of both models in section 5 in terms of storage dynamics. It shows the equifinality within the models applied, since discharge behaves more or less similar, while internal storage behaves quite different. It also emphasizes that a data source such as GRACE may help to select a representative model structure.

Concerning the points on calibration application:

1. In the STREAM approach, we discovered that the amount of parameters was quite high, although only 4 (sensitive) parameters were used in the GLUE assessment. For LEW, we have eventually used the GLUE methodology, only to derive a model structure for the LEW approach with identifiable parameters. This exercise was done on one of the tributaries of the Zambezi (Kabompo), for which some runoff data was available. The recession constants and wetlands threshold, respectively K_q , K_s and $S_{s,th}$ were estimated through a recession curve analysis. D was estimated and the rest of the parameters ($S_{u,max}$, B , $S_{d,th}$) was taken into account in the GLUE assessment so as to find out if the structure merits identifiable parameters. This was a trial-and-error exercise that was applied on several model structures. Q_{max} is a downstream routing parameter and therefore not part of the model structure shown in Fig. 8. Q_{max} is indeed optimized to fit discharges from the runoff station at Lukulu. $S_{u,max}$ from the STREAM model has been determined *a priori* as a function of land use, e.g. $S_{u,max}$ is lower in cultivated areas than in forested areas. It has not been further optimized through calibration. Finally, the Muskingum parameters (used as post-processing of the STREAM runoff output) were manually calibrated. Between Lukulu and Victoria Falls, a clear attenuation and lag of high discharges takes place, which was the basis of the parameter estimation. We will make a clear table that explains for each parameter how it was established.

2. On page 2634 of the manuscript, we indicate that the DEM was up-scaled to 3 x 3 (km)². Indeed, this is the spatial resolution used for the model. We will clarify

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this in the text by rephrasing the sentence about the up-scaling and include that this resolution was used for computations. For the LEW model, we will include a figure that shows the sub-catchments used. The *in-situ* meteo-data (1960-1972) from the GHCN 2.0 database were interpolated by applying the weighted inverse distance method as indicated on page 2632 of the manuscript. We have used monthly numbers since no daily data was available. Moreover, rainfall records show a large spatial variability on daily basis, which would make interpolated rainfall grids rather unreliable taken that daily data would have been available.

3. Parameters in STREAM were indeed considered to be lumped at the sub-catchment scale, except for $S_{u,max}$ and $S_{s,max}$ which were derived from spatially distributed data (land use and elevation data). We shall clarify this in the final manuscript by adding a sentence which mentions that ‘all other parameters have been considered lumped at the scale of the sub-watershed’. We also would like to mention to the referee, that the use of spatially distributed inputs (in particular rainfall) combined with the marked threshold behaviour of processes in the upper Zambezi, can be the cause of local exceedance of thresholds happening more often than in a lumped (or semi-distributed) approach. This was one of the reasons that STREAM was adopted in the first place. However we discovered that the lateral redistribution of surface runoff was a far more important process to take into account. Moreover the computational time required by STREAM was too high to find a model structure with identifiable calibration parameters. Therefore LEW has been developed and applied to the upper Zambezi.

4. The split sample test is indeed used to validate models. We have considered this, however too little data was available to apply this test. Due to the long time scales of processes in the upper Zambezi, the spin-up time of model states can be over a year, which would affect the model performance criteria too much. Moreover, the aim was to compare two models, not to validate a model.

5. Finally, we would like to react on the last point of comment concerning the representation of storage behaviour. On page 2641, we mention that after a sequence of

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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, replenishment of these reservoir-like areas by the first months of rainfall takes place before a reasonable discharge occurs at Victoria Falls. Indeed we have no quantifiable information to support this hypothesis but we observed during our field visit that this phenomenon is indeed occurring in the upper Zambezi. Since the LEW model does show this interannual variability, we expect that LEW represents the true physics better than STREAM.

Concerning the 5 minor technical comments:

1. We will discuss with the editor whether or not the term ‘orthogonal’ is appropriate.
2. We will add the units on page 2631 line 8.
3. D is indeed a constant threshold, however I (the actual interception) is always the smallest of D , P or E_p . We will adapt eq. 1 to include E_p as a constraint for interception.
4. We will replace mereits for merits.
5. The parameters are constant within each sub-watershed but variable per sub-watershed. We will clarify the title of Table 1 accordingly.

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