

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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We are very pleased with the comments of referee Ludovic Oudin. The comments are very clear and we will take them into account in the final manuscript.

The referee’s first remark concerns our selection of the model frameworks. First of all, we would like to clarify that the scope of our paper is not to compare different spatial basin configurations such as distributed, semi-distributed and lumped. The main reason why in the first place, STREAM was selected was because STREAM offers a framework to use spatially distributed data such as rainfall, evaporation, land use, soil type, etc. We considered the inclusion of spatially distributed data to be important, because due to the threshold behaviour that can be observed in the basin’s hydrology, this may improve the simulation of discharge peaks. However, we discovered that

the redistribution of surface runoff is a more important phenomenon, which cannot be included in STREAM and that runoff is also groundwater driven with a relatively long timescale. Also, the computational efforts of STREAM are too large to efficiently include Monte Carlo simulations such as GLUE, and data assimilation procedures. The LEW framework is specifically designed for the redistribution of surface runoff and offers short computation time and therefore proved to be far more suitable for our area of interest. Probably we did not state the objectives clearly enough in the introduction of the papers scope. We shall accurately rephrase the intentions we have with this modeling exercise. We have chosen to mention GRACE specifically so as to clarify in which framework the models were designed. The readers of our paper can then judge whether the model structures chosen are suitable for combination with GRACE data. Moreover in section 5 we show the resulting storage behaviour of both STREAM and LEW and draw a conclusion on the potential of GRACE to conclude which model (structure) is correct. We hope that these remarks answer the comments given in discussion point 1 and 2 of the referee.

Concerning discussion point 3: we agree with the referee that there is quite some complexity in the model structures. However 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. The recession coefficients K_s and K_q in each sub-catchment are 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). Therefore we have confidence that a monthly conceptualization of the complex processes is valid for this river basin.

In discussion point 4, the author of the comment mentions that model parameters cannot be further constrained by GRACE data to better simulate the storage quantity and pattern. We agree that taking into account storage estimates from GRACE in the GLUE procedure will probably not change the storage estimates provided in fig. 12 of the

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manuscript significantly when the current model structures are preserved. However on page 2642 of the manuscript, we mention that theoretically such new data sources will not only constrain the ensemble of possible parameter values but also constrain the theoretically infinite number of possible model structures: if the structures used in this paper are inadequate according to the new data source, then we should look for refinement of the model structure to simulate the system accurately.

Finally, we address the technical corrections suggested by the referee: 1. “p. 2639 Line 9 Typo: replace “mereits” by “merits”.” This will be corrected in the final manuscript. 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”Ě”. Indeed in a small part of the wet season, wetlands in the model become completely inundated, thus the potential evaporation becomes the limiting factor for evaporation. We will revise this statement. 3. “p. 2634: is interception independent to PE, and only depend on rainfallĚ.” The referee is correct. The interception is either constrained by the amount of rainfall (P), by the amount of monthly interception capacity (D) or by the amount of potential evaporation (E_p). We will add E_p in the constraints mentioned in the paper. Note that in the models, E_p has indeed been included as a constraint.

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