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Interactive comment on "State-space representation of a bucket-type rainfall-runoff model: a case study with State-Space GR4 (version 1.0)" *by* Léonard Santos et al.

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Answer to the review comments by Barry Croke

We would like to thank Dr Barry Croke for his detailed analysis and suggestions on the article. They will help improving the quality of the manuscript.

Specific comments

1. Page 9, section 3.1: The issue is not really instability but rather the size of the error in the approximation given by the numerical method. There is instability when the errors grow with time. Yes, this is definitely a problem, but the problem starts before this point. Even if the errors decay with time (resulting in a stable solution), they can be large enough to cause problems, particularly if the decay is sufficiently slow. What is needed is a numerical method that gives a sufficiently small error at the time-step of interest. The reason for going to a finer sub-step is to reduce the error in the numerical approximation, not to avoid instability (essentially, stability is a necessary but not sufficient condition). This is a flaw that exists in the literature, but it would be good to not continue to propagate it. Another point here is that by going to a sub-step calculation, you are making assumptions about how the inputs (rainfall and potential evaporation) are distributed within a time step. Is the rainfall a delta function at the start of the time step, a constant rate over the time step (zero order hold), or something else?

   We agree that stability is necessary but not sufficient. However, the adaptive sub-step calculation method used in this work is designed to particularly reduce instabilities as the sub-step value calculation is based on the difference between two consecutive solutions obtained with different tested sub-step values. To be sure that this method was interesting we compared it to a fixed sub-step Euler implicit method with one hundred sub-steps and the differences between the two were very low. Regarding the second remark on the need not to propagate the confusion between error and instabilities, we will better explain this point in the revised version of the article. About the assumption on the input distribution, we considered the input as constant over a time-step (over one day for the daily model and over one hour for the hourly model). We are aware that this is a simplification of the truth but without more indications at the sub-hourly time-step we decided to keep it constant for the hourly and daily models. We will add this information in the section 3.1 to help understanding.

2. Page 10, section 3.3, line 9: Given the use of a log transform, are there zero flows present, or are all stream perennial? If there are zero flows, how are these handled? Options are to simply ignore them (meaning the model can take any value for time steps with zero flow), or use the two parameter Box-Cox transformation. The later should be generally preferred as this includes assessment of the performance of the model even when the observed flows are zero.

   To handle the zero flow, a small quantity corresponding to one hundredth of the mean flow of the catchment is added to flow in the log transform. This technique was used by Pushpalatha et al. (2012) on the Nash-Sutcliffe efficiency and we adopted it. This will be specified in the revised version of the manuscript.

3. Page 14, line 22-24: This may be due to the sub-step calculation in the numerical integration. This would convert the model to something approaching a continuous time model (using the zero order hold), as in the papers published by Littlewood, Croke and Young (2011; HSJ, 56:3, 521-524) and Littlewood and Croke (2013; Hydrology Research, 44, 430-440). These papers compared a discrete-time model (IHACRES) and a continuous-time model (CT-DBM model in the Captain toolbox), and showed that the variation in the parameter values was significantly smaller for the continuous-time model. This re-emphasizes the need for the distribution of the climate input within a sub-step to be defined.

   This is a very good remark, the production store differential equation resolution can approximate a continuous time runoff input as used with CT-DBM in the 2013 Hydrology Reasearch article. Regarding this approximation, it tends to confirm on a wide range of catchments the result that this paper highlighted. However, we can also explain the difference between x_4 parameters by the higher errors due to operator-splitting approximation in differential equations resolution at daily time-step. The higher errors may introduce differences in calibrated parameter values. This is, in our opinion, a combination of these two modifications that allow the parameters values to be constant across time-steps. In this context, we can admit that the constant distribution of input is problematic but, until now, it is the best approximation that we can use. We will further discuss this point in the article and introduce the cited references.

4. Page 16, Figure 9: There are a couple of outliers in the x3 plot, one with an extremely large difference in the value. Any ideas why this catchment is behaving so differently? Is it a very small catchment?

   Indeed, the two outliers catchments are small catchments (145 and 20 square kilometers area). But, as other studied catchments with a similar area did not face this issue, it is not the only reason to explain this behaviour. This is neither due to the state-space transformation because the parameter differences between daily and hourly transformation also exist with the discrete version of the model for these catchments nor is it due to performances because models are quite good on these catchments. The difference between daily and hourly parameter values may be due to an equifinality between x_1 and x_3 parameters on these catchments. We will discuss the case of these outliers in the articles.

5. Page 16, Figure 10: Obviously there are same extremely large negative values in the KGE values using log transformed flows. This means that some of the models are giving very poor fits. Presumably the mean value for the state space model is just a little below zero? Might be worth including a little more discussion on this?

   The mean KGE' on the log is -0.0825, this negative value is due to some strongly negative KGE' values. To deal with these values that introduce troubles in performances analysis, we will replace the KGE' criteria used in the article by a bounded version of it. This version, bounded between -1 and 1, is calculated like the C_{2M} criterion (Mathevet et al., 2006; IAHS Publ. 307; 211-219) which is

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based on Nash-Sutcliffe efficiency. The formulation will be:

$$C_{2M} = \frac{KGE'}{2 - KGE'} \tag{1}$$

6. Page 17, line 7: Not really correct to say that $nres = 11$ solves the second equation in equation 10. $nres = 11$ gives a value of 1.2511, so it approximates the required value of 1.25 very closely, but doesn't solve it.

   Indeed, 11 is the integer that gives the best approximation for the equation 10. Thus, we chose this integer as the number of stores in the Nash Cascade. We will be more precise in the sentence by writing: "A number of store $nres = 11$ is the best integer approximation to solve the second equation of Eq. 10"

Typographic errors will also be corrected.

Léonard Santos, on behalf of co-authors

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