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Geoscientific
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Discussions

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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General Comments

This paper describes a continuous time state space formulation of a revised GR4J model, using a Nash Cascade for the Unit Hydrograph module. The state space formulation is then compared with the discrete time version of the model. This procedure itself is not new, but application to a complete hydrological model is. The result is interesting: no significant change in the performance of the model, but significantly less variation in the parameter values for hourly and daily time steps with the state space model (figure 9) compared to the discrete time model (figure 8). For applications

where the interpretation of the parameter values is important (e.g. regionalisation), the state space model may give better result. The paper is very well written. The paper would be of interest to model developers across disciplines, and would be a worthwhile contribution to the literature.

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 timestep 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?

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.

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.

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?

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?

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.

Typographic errors

1. Caption for figure 1: "discrete"

2. Page 17, equation 10: error in superscript in second equation (has $(nres-1)^n res$ rather than $(nres-1)^{nres}$).

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