

Interactive comment on “Sub-daily runoff simulations with parameters inferred at the daily time scale” by J. E. Reynolds et al.

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The manuscript deals with the analysis of the time scale dependency of eight parameters considered sensible in a lumped, highly simplified conceptual hydrological model applied to a 90 km² steep tropical catchment in Panama with uncertain data. The slope is 880 m over 17 km of stream length. The runoff-rainfall coefficient is 72 % with more than 3000 mm/year of rainfall over 7 months.

Compared with the spatial scale dependency of parameters few research results have been published so far on the topic, which all indicate that further research is needed. The manuscript presents three open questions, the dominant one asking whether time scale dependency of parameters is predominantly caused by inadequate numerical

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methods. The hypothesis is that it can be easily removed by applying reliable, i.e. error free methods.

There is a clear contradiction in the introduction. It is emphasised that there is a distinct need for flood forecasting in such catchments under consideration which requires the computation of the flood peak flows and their related event volumes. However, the first question raised is whether mean daily flow prediction can be improved by sub-daily time steps; this question seems rather irrelevant for this catchment and objective. The important question is, whether we can predict sub daily flows forecasting the peak flow with parameters estimated with daily rainfall data (as somehow announced in the title). The paper hardly presents novel concepts for analysing the time scale dependency, but reduces the problem to inadequate application of numerical methods; this is completely opposite to the argumentation of former publications. In this respect it must be stressed that the literature review is significantly incomplete. When only very few publications are available and the topic is considered important, the literature review (which does not really exist) must contain more details.

The model applied belongs to the family of highly simplified lumped conceptual models. This model obviously does not contain a compartment to account for overland flow (personal communication with the main author). In other climate regions it could be discussed whether overland flow is expected. For the case study, however, it is safe to assume that it exists. The model is defined by very few equations, but it seems to exist in different versions.

The manuscript comes to the conclusion that the application of a stable numerical method with daily rainfall data produces highly similar mean daily runoff as the use of sub daily rainfall data. This is neither new nor surprising and as stated above of little practical relevance. Peak flows are required instead. In general it could and should be asked whether the many papers written on equifinality or parameter uncertainty or ensemble simulation drew the wrong conclusions for the wrong reasons. The authors very much rely on visual inspection of input data, they correct rainfall as well as flow

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data with some explanations how they did it. Satisfactory explanations why they they did it are lacking. It is well-known which objective methods are available for testing data for homogeneity and consistency, the simplest ones being e.g. double mass curve analysis or multi-dimensional linear/nonlinear regression. The manipulation of rainfall and streamflow data is not transparent and violates unwritten laws in hydrology: never change hydrological data without detailed explanation. Therefore, this subjective manipulation of hydrological data seems scientifically unacceptable.

The choice of the model should have been explained. Why was such an over-simplified lumped model chosen (unfortunately already frequently applied to solve practical problems) for a catchment and study objective where it is obviously inadequate? There are many suitable models freely available in science which use more physically based differential equations and parameters – and solid numerical integration schemes.

The paper is based on two groups of simulation experiments EP1 and EP2. Their description is confusing and difficult to follow, which could be easily improved by using two tables for their definition. E.g., Figure 3 shows once again that the application of a poor numerical scheme leads to highly different results from stable schemes. The figure is based on daily data only; it should be completed indicating the rainfall-runoff coefficients for the different runs. Amazingly the erroneous scheme reproduces the highest flood runoff best. For the numerically stable approaches it is evident, that the model structure applied is not suitable to account for fast surface flow. Flow peaks and volumes of major flood events are reproduced at very limited accuracy. The authors cite most of the relevant recent literature. However, given the contradictory results presented one could expect a more detailed discussion of relevant literature in a separate chapter (as standard). Also, the citation of the chosen references is incomplete and partly erroneous. (citations according to the paper under review)

Kavetski et al (2011) say: “[74] The hydrographs of models M1 and M4 calibrated at 1 and 24 h resolution are shown in Figure 5. Figure 5, in particular, the zoom plots in Figures 5h –5j, illustrates how high-frequency quick-flow processes are obscured by

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the smearing of the forcing and response data, which is inevitable when averaging over larger time scales.”

Of even higher concern is the authors assumption: “However, their models use simple numerical methods at any time step to solve the equations (e.g. explicit Euler).....” This appears to be a most selective and unjustified assumption

Littlewood & Croke (2008) apply a modified version of the model IHACRES. “The software used was v2.1 of IHACRES Classic Plus (ICP) from <http://www.toolkit.net.au/> (Croke et al., 2006)“. Unfortunately, I could not find information about the numerical solution scheme, but the assumption might be correct for this case.

Ostrowski et al (2010): “As model application in this paper is limited to physically oriented, but still conceptual models, we only address modelling approaches with analytical (i.e. error free) solutions.“ Wang et. Al (2009) The authors present the governing equations, but do not explain the numerical method in detail. There is no indication of inadequate numerical methods applied.

The title announces the simulation of sub-daily runoff with parameters estimated on a daily time scale. Given the objective of model application for flood forecasting, one might expect extreme stream flows (m^3/s , or $\text{m}^3/(\text{s km}^2)$), but all results are expressed in runoff depth over the time step applied. This is neither helpful nor in accordance with international conventions, as far as they exist at al (see below). It also aggravates the comparison among different publications.

In fact, the abstract contains strong statements about time scale dependency of hydrological parameters and simulation relying on them. The paper, however, does not provide sufficient evidence that the hypothesis and conclusions are correct.

The paper in general is structured, however it lacks a concise but yet profound discussion of state of science, which should be found again in the discussion of results. There are mismatches between definitions of parameters concerning parameters K1 and K2.

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In the text they have the dimension $[L/T]$, later they are defined as $[1/T]$.

Another distinct weakness of the manuscript is the definition and presentation of terms and units (see above). This is a critical point not only in this paper but valid in general, I refer to the definitions of the World Meteorological Organisation WMO (2008), Table I.2.2. Recommended symbols, units and conversion factors.

Here runoff is defined as an integral of hydrological response to rainfall depth over space and time, its dimension is mm. Streamflow is the momentaneous flow through a cross section, its dimension is m^3/s , or divided by the catchment area $m^3/(s \text{ km}^2)$ $[L/T]$. This is also a message to the editors to insist on standard units (SI, WMO).

The following recommendations are given.

A concise but profound literature review is required The definition of numerical experiments should be reformulated to make it easy to understand. Run identification should be mnemonic. The governing equations of the model applied should be published. The simulation periods should cover identical periods with emphasis on flood events. The variable presented should be consistently mm/h for rainfall and m^3/s or $m^3/(s \text{ km}^2)$ for streamflow. Two figures should be added showing the most extreme flood events in highest temporal resolution within the conditioning (parameter estimation) and simulation period (forecast). Parameters used should be the optimal ones estimated with daily rainfall data, streamflow is based on measured aggregated rainfall for 1h, 3h,

As mentioned above science requires the easy reconstruction of results. This sincere basic principle is violated more or less continuously in hydrological sciences. Therefore:

it is urgently required to publish the hydro-meteorological data as well as the model as public domain. Otherwise such publications should be rejected in general in the future. Hydrologists with secrets have no place in science. If you cannot publish the data and model, do not publish! (My personal conclusion is: Hydrological models are located on

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the fringe between indispensable tools and unneeded dangerous toys).

References:

WMO (2008): Guide to Hydrological practices (Volume I), Publication 168, Geneva, Switzerland, ISBN 978-92-63-10168-6

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