Interactive Comments and Responses on "Sub-daily runoff simulations with parameters inferred at the daily time scale" by J. E. Reynolds et al.

M. Ostrowski (Referee)
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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 km^2 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 methods. The hypothesis is that it can be easily removed by applying reliable, i.e. error free methods.

Reply: We greatly appreciate the reviewer for careful reading and valuable comments.

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).

Reply: We realize that we formulated the text a bit awkward, which caused the reviewer to think that there was a contradiction. In the revised version we will make it clearer that our main motivation was to produce forecasts at sub-daily time scales, when data are available at daily time scales. The goal was achieved in two steps, first, thanks to the availability of the short scale data (which is a very rare case in Central America) which allow us to make cross scale comparison of model performance as well as the stability of model parameters with change of time scales in the paper; second, we examine the performance of sub-daily simulation using daily input data. In the revised version we will add some results (Figures and/or tables) to address more of the model performance on sub-daily scales when using daily input. We will also remove the question and results regarding the accuracy of mean daily flow predictions when using sub-daily or daily input data to keep it relevant to the main objective of our study.

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.

Reply: Thank you for the comment and sorry that we failed to state more clearly the novelty and contribution of the study in our original submission. In the revised version we will better explain the novel aspects, namely the analyses of the time scale dependencies by running the model always at the same time step but with input data series aggregated at different time scales. This is a simple approach to separate time step dependencies due to input resolution and due to numerical issues we have not seen in the literature before. Our interpretation from the literature is that the time scale dependencies of the model parameters are not fully understand and this paper gives insights on how to minimize these dependencies by looking at the numerical methods applied in modelling. In the revised version we will include more details of the previous publications, to do not overlook their arguments regarding the probable causes of these dependencies.

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.

Reply: In the revised version we will extend the part on previous work, although a full review of course is beyond the scope of this study.

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.

Reply: The reviewer is right, we used the HBV model as a typical example of the rather simple bucket-type water balance models, which are frequently used for water resource planning. In such models one has to be careful when naming certain flow pathways. Basically there are different outflows with different response times (quick/slow), but one should not link these to specific flow pathways. The HBV model has been frequently used in catchments which are similar to that used in this study with general good results (Lindström et al., 1997).

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.

Reply: We realize that there might have been a misunderstanding. To forecast flood normally sub-daily or even hourly time step is needed, but normally daily day is more available, what we try to show in the study is whether the parameter sets inferred at daily step were able to reproduce the flow dynamic at finer time scales. But we failed to make this important message clear enough. In the revised version we will remove the results regarding the ability of the model to produce mean daily runoff with input data at different time scales to keep it relevant to the main objective of our study.

In general it could and should be asked whether the many papers written on equifinally or parameter uncertainty or ensemble simulation drew the wrong conclusions for the wrong reasons.

Reply: Honestly we are not sure what exactly in our manuscript the reviewer is referring to with this general statement.

The authors very much rely on visual inspection of input data, they correct rainfall as well as flow data with some explanations how they did it. Satisfactory explanations why 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.

Reply: We realize that we have to give more detailed explanations of the quality controlled of the rainfall-runoff data performed in our analysis. We visually compared the 1h runoff and rainfall data to evaluate the consistency of them on event scale. During this quality control, the rainfall data were not corrected and assumed as true since no additional information about uncertainties was available. However, in very few cases (0.52% and 0.56% of the high and low flows in the whole discharge data)[1], runoff responses without any precedent rainfall or with larger volumes than their precedent rainfall were removed and set as missing values before the parameter inference phase. For those cases, we thought that the areal rainfall estimates may have not characterized correctly the true rainfall inputs to the catchment or that the runoff data were incorrect at those time steps, but we didn't know which of the previous was the one incorrect (or if both were). Regardless if the rainfall data were correct or not, the model needs these data to be driven. The only practical solution to do not affect the parameter values to be infer due to the evident uncertainties in the data was to not take into account the runoff data in those steps. Even if the runoff data were correct and we include them for the parameter inference phase, there is no physical or empirical model that could produce accurate runoff simulations at those time steps if the driven data do not characterize correctly the rainfall inputs to the watershed. Omitting these errors in the data would have been an additional source of uncertainty in the simulations. In the revised version we will give more detailed explanations of the quality controlled performed in our study.

[1]By high flows, we refer to those with a probability of occurrence below 10% (Q>=13.54 m3/s) and by low flows we refer to those with a probability of occurrence above 10%. Maximum and minimum flows at 1 hour steps are 1,028.86 m3/s and 0.386 m3/s respectively.

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.

Reply: In the revised version we will make it clearer that the choice of HBV is based on the following reasons: (1) the input data to the model was available in the study catchment (which is very often the main criterion to select simple models in the Central American region), and (2) the model have reported good performance in the study region, and (3) the choice of the model fulfils the requirement of the study objective. Many studies have previously used this model for similar applications as the one herein (e.g. Grillakis et al., 2010; Kobold and Brilly, 2006; Wöhling et al., 2006), which gave us enough confidence for using it to accomplish the purposes of our study. A more complex model (i.e. a physically based model) was not chosen in our study for two reasons. First, because this would have meant more parameters to calibrate, or some to assume as constants, leading up to more uncertainties in the simulations, especially outside the parameter inference period. Second, because this type of model usually requires other data than precipitation and evaporation which are commonly not available in Central America. Simple bucket-type water balance models, such as the HBV, are frequently used and many researches would agree with the statement that the limited data often does restricts the use of more complex models

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.

Reply: Thanks for your comment and sorry that we failed to make it clear and easy to follow in our original submission. In the revised version we will remove the acronym EXP as suggested by Referee #3 and we will add two tables for defining the experiments as you suggested.

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.

Reply: In the revised version we will complete Figure 3 indicating the rainfall-runoffs coefficients for the different simulations and the values of the parameter set used.

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.

Reply: Thank you for your comments and sorry that we failed to explain clearly experiment 1. All the simulations made in this experiment were made with the same parameter set which was inferred at daily steps using the explicit Euler method. The intention of experiment 1 was to show that erroneous conclusions in parameter inference than can be made if the explicit Euler method is used at large steps. It would have been surprising if the numerically stable approaches would have reproduced accurately the flow peaks using this parameter set.

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).

Reply: This will be addressed in the revised version of our manuscript.

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 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.

Reply: We will addressed this issue in our review version of our manuscript and will remove our statement about "their models use simple numerical methods at any time step to solve the equations" since this does not applies for all the chosen references.

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 (m3/s, or m3/(s km2), 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).

Reply: We will change the units in all the figures where we expressed runoff depth over time step applied to m3/s in the revised version of our manuscript.

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.

Reply: We will carefully reformulate the abstract and conclusion chapters to make statements consistent throughout the manuscript.

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.

Reply: This will be addressed in the revised version of our manuscript.

There are mismatches between definitions of parameters concerning parameters K1 and K2.
In the text they have the dimension [L/T], later they are defined as [1/T].

Reply: Thanks for your comment and sorry for the mistake in our original submission. We will correct the dimension of parameters K1 and K2 to [1/T] in the revised version of our manuscript.

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 m3/s, or divided by the catchment area m3/(s km2) [L/T]. This is also a message to the editors to insist on standard units (SI, WMO).

Reply: In the revised version of our manuscript we will present the units of rainfall and discharge as mm/h and m3/s respectively.

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 mnmonic.
- 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 m3/s or m3/(s km2) 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, : : :..

Reply: Thanks for your recommendations. They will be fully addressed in the revised version of our manuscript.

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 the fringe between indispensable tools and unneeded dangerous toys).

Reply: While we agree with you that this would be optimal, one has to be realistic. Regarding the data, we like many other studies are limited by the data owners to distribute them. We will describe the data source clearer so that everyone who might be interested could ask the data owners for access to the data. Regarding the HBV model, the source code of the core model part is de facto open source and there are numerous versions of the HBV model. We freely share our version as executable (http://www.geo.uzh.ch/en/units/h2k/services/hbv-model), however, we simply to not have the resources to make the source code of the entire model package openly available and still to maintain support to users and a version control.
At this point, we have permission to make public the 1 h rainfall-discharge data, but not the daily pan evaporation data used in our study. Despite of the previous, we can still facilitate the long-term monthly values of pan evaporation data of the same station (data freely available at the data owners website) (HIDROMET, 2015), that way the model can still be run for modelling at all the time scales chosen in this study. For this study catchment, the runoff predictions are expected to not vary considerably when using long-term daily or monthly pan evaporation data to drive the model.

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