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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Characterization of event water fractions and transit times under typhoon rainstorms in fractured mountainous catchments: Implications for time-variant parameterization" *by* Jun-Yi Lee et al.

Anonymous Referee #2

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In the manuscript "Characterization of event water fractions and transit times under typhoon rainstorms in fractured mountainous catchments: Implications for time-variant parameterization" by Lee et al., the authors aim to quantify temporal variability of transit times and event water fractions and to identify controls thereon in a Typhoon-dominated region. They do so by calibrating the TRANSEP model to individual events and then comparing the model parameters and outputs under different conditions. The overall objective of the study is, in principle, worthwhile and the presented data set very interesting. I nevertheless struggle to get enthusiastic about the analysis for several

reasons:

(1) While TRANSEP was a great tool at the time of its development, there has been ample progress in the field of transit time modelling in the 16 years since. Although there is nothing inherently wrong with the use of TRANSEP, it remains elusive to me why the authors chose not to use a simple conceptual model together with the concept of SAS-functions. Calibrating and running such a model, which typically does not have more than 10 parameters, for the entire study period (i.e. also on non-typhoon days) has several advantages. Firstly, the SAS-function formulation directly gives temporally-varying TTDs for each time step as output. From these event fractions can be easily inferred as well. This approach would give a more complete picture of how TTDs are varying throughout the year. In addition, analysis of the model storage dynamics will allow the authors stronger support for many of the interpretations given in the current manuscript, where it is currently essentially speculated that changes in MTT are somewhat related to the level of catchment wetness. Secondly, the calibration in such a continuous model would be more robust, as now the 12 (?) parameter model is individually calibrated against each event, whereby each event only consists of a few data points. Thus, the degree of freedom in the model application unreasonably high. For a continuous model at least the number of stream flow data points to calibrated the model against would considerably increase (while the number of O-18 event samples will remain unchanged).

(2) The first research hypothesis cannot be tested with the available data and the statements made in the conclusion section referring to this hypothesis are thus not supported by the results. Obviously, the authors use results from previous studies to extend their data base and to allow for such an analysis. However, it remains completely unclear which studies these are and how they were chosen. Similarly it remains unclear, which parts of the results discussion and conclusion sections refer to the authors own work and which to other work. A much clearer separation is needed here.

(3) The calibration procedure is not described in sufficient detail. It is mentioned that the

best parameter sets were retrieved, based on the two KGE values. How was this done? Per definition, a set of pareto-optimal solutions does not have a single “best” solution. Furthermore, table 4 lists upper and lower limits of parameters. What are these? The set of pareto-optimal solutions? The same question applies to figure 3 – what are the shaded areas around the modelled streamflow? Why are such uncertainty intervals not provided for the O-18 model results in that figure? Why are this uncertainties not considered in figures 5 and 6?

(4) It remains completely unclear which rainfall O-18 data were used in the analysis. Data from 4 sampling locations were available. Were they averaged? Was one chosen?

(5) For most figures and tables: axis and captions need to provide all units and need allow the figure to be standalone. Currently, units are frequently missing and the captions remain unclear.

(6) Figure 2 is redundant with figure 3 and can be removed.

(7) The level of English is rather poor, making large parts of the manuscript difficult to read.

Other points:

p.1,l.11; p.2,l.51: really? What about e.g. Asano and Uchida (2012) or Hale and McDonnell (2016)?

p.2,l.33-35: time-domain convolution and spectral analysis are mathematically essentially equivalent. They cannot be seen as different methods.

p.2,l.35-37: please rephrase – not clear what is meant.

p.2,l.42: “all”? what is meant by that?

p.8,l.237: repetitive – can be removed

p.8,l.242-243: how can you with only 2 stream sampling sites make any statement about the influence of catchment area? What is meant by “extending to 100km2”?

p.8,l.245: which conflicting results?

p.9,l.254: this statement is not warranted! There is nothing that is constant at a value of 0.8 above >30mm (figure 5b). Rather, what can be seen is that there is a lot of variation between $\sim$0.2 and 0.8.

p.9,l.255: the number of data points is too low to call this a “limit”

p.9,l.263: what does this sentence mean?

p.9,l.272-277: likely, but speculative here as not shown by any type of data.

p.10,l.288-299: not sure how this links to this study

p.10,l.302-307: this is very confusing. What do you mean by “sceptical?” The results are all similarly pointing towards an inverse relationship: the higher wetness, the smaller alpha.

References:

Asano, Y., & Uchida, T. (2012). Flow path depth is the main controller of mean base flow transit times in a mountainous catchment. Water Resources Research, 48(3).

Hale, V. C., & McDonnell, J. J. (2016). Effect of bedrock permeability on stream base flow mean transit time scaling relations: 1. A multiscale catchment intercomparison. Water Resources Research, 52(2), 1358-1374.

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