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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.

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We thank reviewer 2 for the insightful comments which focus on model selection and calibration procedure. In this revised version, we will strengthen the manuscript by adding the rationale why we selected TRANSEP and how we calibrated parameters. The following is a point-by-point response to the reviewer's comments. All corresponding changes in the revised manuscript are underlined.

In the manuscript "Characterization of event water fractions and transit times under

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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 temporallyvarying 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).

Response: Many thanks for this professional comment raised by Reviewer 2, who concerned three issues: (1) the reason for choosing the time-invariant TRANSEP as a tool, not time-variant SAS-functions; (2) Why not using a SAS-function to infer the reasons of MTT changes; (3) the reason why calibrating the events individually instead of a long-term continuous period. We replied the comments below. We almost revised the whole introduction and discussion and part of conclusion to clarify our main theme. Because the revision is of great extent, only the main corresponding changes are attached here.

For the first comment, to our knowledge, time-variant models were developed later and got popular rapidly because they can render TTDs temporally, by which some clues to storage dynamics throughout a given period can be inferred. However, our study tried to compile all MTT_{ew} studies associated with event water; among the few studies some of them were done before time-variant models were developed. So far, Jasecho's work explored the insight of young water fraction in different landscapes. They concluded that young streamflow is less prevalent in steeper landscape due to long flowpaths and high permeability due to fractured rocks. In their study, substantial event water (fast runoff) fraction in steep catchments during rainstorms was observed and recognized. The new found territory shows the necessity of understanding of mean transit time (MTT_{ew}) and fraction (F_{ew}) of event water during rainstorms [Line: 38-42]. In this regard, compiling all MTT_{ew} studies associated with event water to broadly investigate the potential landscape and climatic controls in different regions is essential for further model development. However, the accumulation of time-variant study at event scale is relatively few. Therefore, we used the time-invariant TRANSEP as our modeling work in comparing with the compilation.

Another reason that reviewer 2 promoted SAS-function is that the analysis of the model storage dynamics can support many of the interpretations; for example, changes in MTT are somewhat related to the level of catchment wetness. We agreed that the SAS-functions indeed have some advantages in interpreting the dynamics of rainfall-runoff transfer. Only one question is that most SAS-functions are presumed, which means

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that we can apply various SAS-functions from different philosophies onto catchments and events to straightforwardly test the hypothesis (Kirchner, 2019, we cited it in our manuscript). Alternatively, the exploration between the temporal parameter sensitivity during rainstorms can help us developing hypothesis. For example, why the storage behavior changes from plug flow to preferential flow with the increase of rainfall intensity? Such behavior also infers the mixing degree in storage. In fact, our study in the present form provides us some thinking in storage dynamics, particularly for the role of subsurface storage. It seems that the mixing degree and allocation of different water age in subsurface are more prevalent in our steep catchments. We are currently taking a further step into the theory with a second study.

The third comment is about the calibration procedure. Using a long continuous period has the advantages of lowering the freedom effect. However, this kind of calibration procedure would give a universal parameter-set which can simulate the whole period unbiased (depending on the selection of performance measures); nevertheless the rainstorm period would be overlooked due to the relatively fewer observations. Therefore, some studies, particularly for event simulation, calibrated the parameters among individual events (Weiler et al., 2003; Segura et al., 2012, the two were also cited in our ms). Through the calibration for individual event, the corresponding parameter-set of each event can be used for inferring the rainfall-runoff processes. Reviewer's another concern is freedom effect. This issue still exists and is inevitable for the available data during an event. The only thing we can do for reducing the potential freedom effect is to increase the sampling frequency. Simultaneously taking stream discharge and sequential isotopic data into account can more or less reduce the freedom effect.

(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.

Response: As we replied above, the sections of introduction and discussion were totally revised for clarification. As reviewer pointed out, the original hypothesis 1 and the corresponding statements in conclusion were improper and have been removed in this revision. The goals of this study were rephrased as: "Specifically, we aim to investigate: (1) the control of hydrometric input (rainstorm) on MTT_{ew} and F_{ew} in steep catchments, and (2) the variability of TTD-associated parameter sensitiveness during rainstorm. This study on the event water MTT of extreme rainstorms in mesoscale mountainous catchments as provided here may shed new light on MTT model development in the future." in Line: 66-69. Besides, the data sources of the compilation were listed in supplementary Table S3. We addressed it in Line: 255.

(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?

Response: We rewrote the text of the calibration procedure (Sect 2.4). First, we used KGEQ > 0.8 as the threshold of reasonable streamflow simulation. Second, we selected the parameter sets on the Pareto front (KGEQ vs KGEC) as representative simulations. Only a parameter was taken as the 'best-performed' parameter set that has the highest KGEC on the Pareto front. In Table 4, upper and lower limits of parameters are the set of Pareto front. In Fig. 2 (original Fig. 3), the shaded areas are streamflow (cyan in Fig. 2b and e), event water (gray in Fig. 2b and e), and O-18 (yellow in Fig. 2c and f) of the representative simulations. We provided the uncertainty intervals in yellow shaded areas, but they are too narrow to be seen. Also we added error bounds in Fig. 4 and 5. The above descriptions were shown in Line: 172-176.

(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?

Response: We only applied rainfall O-18 data in P1 onto the modeling work. Samples of the 4 sites were intermittent because of bad traffic condition during typhoons in this remote and roadless catchments. We clarified it in Line: 105-106.

(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.

Response: We checked all the figures and table and added axis and units wherever needed. Fig. 5 and 6 (original Fig. 6 and Fig. 7) were corrected. Besides, the captions were rephrased so that the figures can be standalone.

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

Response: As reviewer suggested, the original Fig. 2 was merged to Fig. 3. The corresponding text is also re-written into the next paragraph.

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

Response: We carefully checked the grammar error, sentence structure, and typos in this revision and invited a professional editor to refine this manuscript for readability.

Other points:

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

Response: Reviewer is right. Some MTT-associated studies were undertaken in steep catchments. Therefore, the opening sentence is rephrased as: “Transit time estimation has rarely been done for violent rainstorms (e.g., typhoon) in steep and fractured mountainous catchments where the range of transit time, potential controlling factors, and the validity of time-invariant parametrization are unclear. Characterized by steep landscape and frequent typhoon invasions, Taiwan provides great opportunities for inquiring into the above questions.” [Line: 11-14]

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

Response: The original statement is not clear and a little wordy. The sentence now reads as: “Typically, TTDs are mainly derived from an assumed functional form (e.g., exponential or gamma distribution) whose time-variant and time-invariant parameters are estimated from passive tracers and hydrological data (McGuire and McDonnell, 2006; Harman, 2015). While the time-variant parameterization with different methodologies is still under development recently (e.g., Harman, 2015; Soulsby et al., 2015), the time-invariant parameterization has been applied across many regions since 1990” in this revision. [Line: 32-36]

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

Response: see above.

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

Response: Removed.

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

Response: 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”?

Response: Reviewer is right. We should not have leapt too far based on only 2 stream sampling sites. The original sentence has been eliminated and the whole paragraph is revised to avoid the arbitrary statement.

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

Response: Larger catchments tend to have longer travel time because of longer flow path, but the MTT_{ew} in our sites is short. It just shows that the control of catchment size on MTT needs further studies. We rephrased the sentence as: “However, Segura et al. (2012) concluded a positive relationship between catchment area and MTT_{ew} in small gentle catchments in Quebec. While comparing our MTT_{ew} with the above-mentioned studies (Weiler et al., 2003; McGuire and McDonnell, 2010), we found a controversial relationship.” for clarification. [Line: 251-253]

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 0.2 and 0.8.

Response: Agreed. This paragraph and the original Fig. 5 have been removed. Instead, we used the compilation to demonstrate the relationship between catchment size with MTT_{ew} and F_{ew}, respectively. Also, a new Fig. 4 was added. In this revised paragraph, we found that there is no simple and clear relationship between MTT_{ew} with catchment size, slope, and rainstorm size. Also, F_{ew} shows that further works are needed in able to identify the controls on catchment structure and rainstorm characteristics. This identification would be helpful for the upscaling from observed micro-scale studies to unobserved meso- or macro-scale catchments. On the other hand, the unclear relationship with catchment size implies that perhaps catchment structure (e.g., flow-path length, flow-path gradient, and subsurface permeability) might be a more important control than catchment size, not only for MTT of total streamflow (Tetzlaff et al., 2009; Asano and Uchida, 2012; Hale and McDonnell, 2016), but also for MTT_{ew}. The new paragraph is in Line: 254-272.

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

Response: Replied above.

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

Response: Removed.

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

Response: The whole discussion is revised completely. The statements without support by our data were removed.

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

Response: This paragraph was thoroughly revised to demonstrate the relationship between Few and rainfall intensity under dry and wet conditions. Now it read as: “Brown et al. (1999) argued that an increase of event water delivered from throughfall and shallow subsurface flow is directly related to rainfall intensity. Such an interpretation is also supported by Segura et al. (2012), but it remains unclear under different antecedent wetness. Recently, Kirchner (2019) developed an age tracking benchmark model to show that high antecedent wetness increases the partitioning of rainfall that reaches the stream resulting in high F_{ew}. However, Muñoz-Villers and McDonnell, (2012) applied TRANSEP onto tropical montane catchments and found that the event water contribution decreases with an increasing antecedent wetness condition. They argued that if high permeability of soils and lithologic substrate can lead to vertical rainfall percolation and recharge of deeper layers, the rainfall-runoff responses could be dominated by groundwater discharge, rather than shallow lateral pathways. Although our wet antecedent wetness cases (blue dots) also show a decrease of F_{ew} with the increase of antecedent wetness, it would be hasty to jump into a conclusion with only 3 sampled cases. The controversy of F_{ew} responding to antecedent wetness, therefore, still needs further investigations including a thought on the fracture systems in lithologic substrates, like Gabrielli et. al. (2012) suggested.” [Line: 288-298] *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.*

Response: Sorry, it was the sentences that caused the confusion. Actually, what we wanted to say is: "an inverse relationship: the higher wetness, the smaller alpha" as the reviewer pointed out. Now, the sentences are: "When rainfall transports without dispersion, like plug flow, a large α would be determined. For a catchment with $\alpha = 1$, a linear or well-mixed reservoir could be identified. As $\alpha < 1$, the catchment is more likely to exhibit a relatively high degree of nonlinearity (i.e., high initial peaks and longer tails in the TTDs), indicating preferential flow paths are rapidly activated and quickly route water to the catchment outlet." for clarification. [Line: 302-305]

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