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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.

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A transfer-function hydrograph separation model is used to examine event water fraction and transit time of typhoon rainstorms in steep catchments in Taiwan. Few studies of isotope hydrograph separation that involve event water fractions and transit times in high rainfall, subtropical, and steep catchments have been reported in the literature. In addition to a novel catchment setting, a time-variable sensitivity analysis is used to infer processes and controls on event water fractions and characteristics of event water transit time distributions. The results are placed in context with data generated

from literature values and show that rainfall intensity influences event water fractions and characteristics of the transit time distribution. The primary innovation of the study is the application of an existing model to a new setting and the event-dependent and time-variant sensitivity analysis. The study provides additional examples of a model, i.e., a transfer-function hydrograph separation model, that is not widely used, but has a place in the evaluation of event-based isotopic data to improve hydrologic process understanding. The authors conclude the manuscript with a conceptual model that synthesizes hypothesized footpaths and processes across a range of rainfall intensities. In general, the manuscript makes nice contribution at improving the use and interpretation of transfer-function hydrograph separation models.

Response: We appreciate that reviewer 1 who generously shares his/her expertise in this field fully understands our merits and gives constructive comments for this study.

General comments

However, the discussion is fairly one-dimensional (e.g., mostly focused on how rainfall intensity influences event water dynamics) and misses opportunities to connect the time-variable nature of the sensitivity analysis to time-variable transit time studies (e.g., storage selection ideas).

Response: Thanks for this comment. In this revision, we discussed that rainfall intensity influences not only event water but also pre-event water dynamics. We also elucidated the connection of our sensitivity analysis to the dynamics of both. Specifically, we thoroughly revised the introduction and discussion in accordance with the comments. The unrefined hypothesis (hypothesis 1) has been removed in introduction. The relevant variables (e.g. catchment size, slope, and antecedent wetness) were discussed in the revised section 4-1. More information about fracture system in our catchments were addressed and some conclusions without evident support by our study were eliminated (like hypothesis 1). We connected our sensitivity analysis to time-variable transit time tightly in the revised section 4-3.

In addition, the comparison with catchment structural controls from the literature needs some improvement. For example, little information is provided about the fracture system in the catchments and the authors assume that is part of the reasoning for differences compared with literature values when it could very well be related to soil depth distributions or other aspects of catchment structure. In other words, the conclusions related to bedrock fractures and preferential flow could be strengthened with more evidence that these processes are important over other controls. The authors also compare their transit time findings with those in the literature when some of the literature studies did not examine event water transit time, but rather transit time of total streamflow.

Response: We advanced structural controls of both the literature and our sites. With collected three available structure-associated variables (catchment area, catchment average slope, saturated hydraulic conductivity), we were able to compare the catchment controls that influence MTT_{ew} and F_{ew} between various literature. Unfortunately, the relationships from this compilation is not clear. Only a few of them positively correlates to catchment size. We comprehensively discussed it in the revised section 4-1. As for depicting the fractures in our system, we presented the data of the boring logs in Figure 1 and added the descriptions in Line: 75-78. The revisions can also be found in replying the following Specific Comments.

This should be sorted out better throughout the manuscript. The manuscript also requires revision for readability, grammar (plural vs. singular, verb tense, missing "the," etc.), and sentence structure. I found the manuscript not easy to read, but I could follow the general ideas quite well. I did not note all the editorial problems because they are numerous. I suggest a professional editor.

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

Specific Comments

Line 26: Suggesting that the transit time "implies information" about something is not quite right. It is the investigator who makes a linkage and implies something about process. Please revise.

Response: We replaced the term, "implies information" with a statement "…is a single integrated measure at the outlet, which…" for clarification in Line: 27.

Line 29: not only the mean, but other characteristics of the transit time distribution as is discussed in the manuscript (e.g., shape parameters of distributions).

Response: It was rephrased as: "…characteristics of transit time distribution (TTD, e.g., mean, median and shape parameters of distributions)…" to elucidate all characteristics in Line: 30-31.

Line 32: Please be clear about what is meant by "event driven pollution." Perhaps give an example.

Response: The paragraph has been rewritten and the term has been eliminated.

Line 35: It is not clear to me what the authors mean when they say "tracer-aided rainfall-runoff models." I would also suggest thinking about how the concept of storage selection fits (e.g., Harman 2015).

Response: We revised the introduction thoroughly in accordance with this suggestion and the reference. Now, the sentence is rephrased 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." Line: 32-36.

Line 37: not clear what is widely applied.

Response: As replied above.

Line 41: are lacking in hydroclimatic diversity?

Response: The term is improper and thus removed. We want to express that there are many studies associated with mean transit time of event water so we compiled all those data from different landscape and climatic regions in hope to improve the understanding of the controls. This paragraph has been revised in Line: 51-62.

Lines 47-48: This paragraph is convolving the idea of transit time in a general sense (e.g., total stream discharge) vs. event water transit time. For example, I do not believe that McGuire et al. or Tetzlaff et al. ever mention event water. Please keep these as distinct topics.

Response: We carefully used the two terms: event water transit time and transit time of total discharge in this revised paragraph. We also checked the usage of terminology throughout the whole manuscript.

Line 49: Sentence beginning with "Besides" is unclear.

Response: The whole paragraph has been re-written and the "Besides" has been eliminated.

Line 51: What is a "general picture of global MTT"? Please clarify.

Response: The whole paragraph has been re-written and the "general picture of global MTT" has been eliminated.

Line 58: Catchments that are compared have comparable mean slope. The evaluation of this hypothesis seems rather weak. Also, other the catchments that are compared have fractured bedrock as well (e.g., the Oregon site, see Gabrielli et al. 2012).

Response: We recognized that some catchments in Oregon and New Zealand are as steep and fractured as our catchments. The original hypothesis 1 may be a step too far as rethinking about is. We re-wrote this paragraph. Now, it reads 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 a 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.

Line 67: Given that one of the hypotheses of this study is related to bedrock fracture flow and the preferential nature of flow paths contributing to streamflow, I would recommend provide additional background/context in the site descriptions. Any information the authors can provide to help provide convincing arguments on controls on the event water dynamics.

Response: Thanks for the suggestion. Reviewer 1 has mentioned this point in the main concern. We addressed the fractured characteristics in relevant sections and revised Figure 1. In this revision, we added more site descriptions associated with fractured lithology. Now it reads as:" Boring logs show that the lithological formations are highly fractured from ridges to streams (Fig. 1b, provided by Taiwan Central Geological Survey)." in Line: 75-76.

Line 68: Inceptisols and Entisols (spelling) are plural. Also, are the near-stream soils one of these orders? That is not clear.

Response: Corrected. We re-wrote this sentence as "Inceptisols with depth less than 1.0 m cover most of the steep hillslopes, while Entisols that have high permeability with soil depth around 1.5 m lie mostly in the valley bottom (Zehetner et al., 2008)" in Line: 76-78.

Lines 97-98: While the supplement describes how spatial variation of the isotopic composition was assessed, it is so critical given the size of the catchments that some of those details should be included in the main text. Further, the assessment provided in the supplement is only based on two storms and from collectors are relatively low elevations. Most of the area of these catchment is above 300 m and altitude effects on *rainfall are known to vary by about -0.2 per mil per 100 m. This is should be addressed in the manuscript.*

Response: Thanks for the reminder. We recognized that altitude effects on rainfall is relevant to mountainous catchment study. In our cases, the relief is around 700m which may induce considerable altitude effect in the isotopic composition of rainwater. However, our previous studies showed that tropical cyclones bring heavy rainfall of violent circulation (well-mixed vapor) so overwhelming that cyclone effect (circulation) dominates the spatial and temporal rainfall patterns. Spatial heterogeneity is still distinct, but compared to orogenic rainfall, it is relatively homogeneous. As for the isotopic composition of rainwater, we conducted rainwater sampling in remote and roadless environments as near-real-time as possible. Our results show that during typhoon periods, the isotopic composition of rainwater might not closely follow altitude effect and the difference among the 4 sites are relatively small. Fischer et al. (2017) also reported the absence of altitude effect in a catchment in Switzerland (relief is 600 m), especially in events with changing wind direction. As they suggested, it is the interaction of atmospheric circulation and the topography which results in the lack of altitude effect in the isotopic composition of rainwater. We added the following sentences in the main text to describe the spatial heterogeneity of rainfall and the isotopic composition of rainwater.

The sentences are: “The spatial heterogeneity of rainfall and the isotopic composition of rainwater were examined. Typhoon-induced rainfall is short-lived, intense, and its spatial heterogeneity in meso-scale catchments is not considerable, because the strong circulation induced by typhoon becomes a predominant control on precipitation over landscape effect (Huang et al., 2012). Our investigation of rainwater isotopic composition, though sparse, also showed an indistinct spatial heterogeneity. Details of the investigation could be referred to in Supplementary Material (Fig. S1).” In Line: 106-111.

Line 129: This is confusing to me. Why are the amounts and composition assumed

a uniform value when you have data on the time variation and the model uses this variation to compute the event water fraction? Does this defeat the purpose of the TRANSEP model?

Response: Sorry for the unclear description. We rephrased it as: “Note that the time-series amount and isotopic composition of rainwater collected at P1 are used for p and Ce” in Line: 141-142.

Line 230: Discussion

Response: Corrected.

Lines 242-43: Can you really claim that catchment area is not “a control” with only two catchments? Your results support previous studies, but your results do not “indicate” that.

Response: Yes, the reviewer is right. We cannot really claim that catchment area is not a control. What we intended to say is that the effect of rainfall intensity is becoming a more important control over catchment area as rainfall amount increases over 200 mm. We rephrased our statement in this revision.

Line 270: I cannot find that McGuire and McDonnell examined the correlation between rainfall intensity and MTTew. You could say that it is suggested by data presented in McGuire and McDonnell.

Response: Agreed. We rephrased the sentence as: “McGuire and McDonnell (2010) studied the runoff dynamics in H.J. Andrew Experimental Forest in Oregon through successive rainstorms and concluded that the subsurface contributing areas extend far upslope during rainstorms, and the reduction of MTT_{ew} was therefore implied.” in Line: 283-285.

Line 277: The term squeezing is not clear.

Response: We replaced “squeeze” with “reduce” in Line: 261.

Line 287: Instead of perplexed, how about "there was no clear relationship?"

Response: Thanks for the rewording.

Line 300: I think some caution is needed here and throughout the discussion where event water results from this study are compared with other studies when some of the other studies focused on transit times and distributions that represent all streamflow not just the event water. These distributions are likely very different and not comparable. The discussion also never addresses what is unique about typhoon systems. This seems like one of the main contributions of the study, but it is not made clear in the discussion.

Response: Thanks for bringing up this issue. We know that the MTT in event water, base flow, and the total discharge mean differently. We used the terms with care in this revision. Because the discussion has been revised thoroughly, we do not post the corresponding revision here to save space.

Line 307: What does a "fixed flow path without dispersion" mean?

Response: "Fixed flow path" can be misunderstood. We changed the phrase to "When rainfall transports without dispersion" for clarification in Line: 302.

Line 315: What does a "internal operation in simulation" mean?

Response: Rephrased to "the interaction of model functions" in Line: 312-313.

Line 324: sensitivity

Response: Corrected. The sentence has been rephrased, see below.

Line 325: "the timing of dominance of a given parameter or a process" is not clear.

Response: We tried to clarify it in this revision. Now it reads: "Since parameter sensitivity likely varies depending on different wetness conditions, the change of parameter sensitivity within an event should be demonstrated for understanding the dynamics of

the model and the underlying processes, such as the timing of dominance of a given parameter or a process." in Line: 321-323.

Figure 3: Please include error bounds on isotopic simulations too. Also, be consistent with label and label subplots with letters a through f.

Response: Error bounds are included on isotopic simulations, but they are quite narrow due to simulations with high similarity. The label has been corrected.

Figure 4: Move y-axis label of sites to the left of the precipitation.

Response: Done.

Figure 5: provide slope ranges for gentle and steep.

Response: Added. Note that the original Figure 5 was moved to Figure 4 in this revision.

Figure 7: Label y-axis for subplots a. Also, please add a description of the dashed vertical line in these graphs.

Response: The y-axis label is added. "The dashed vertical lines represent the timing of flow peak" is added to the caption for clarification. Note that the original Figure 7 was moved to Figure 6.

References: Other published work that is relevant and could be added to the literature values in the analysis is by Mosquera et al. (2018).

Response: Thanks for the references. This insightful study provides a way to test the 'reality' instead of only 'reliability' of model structure via one more proxy. Besides, it also shows that model structure should not be presumed, but need assessment for identification.

Gabrielli, C. P., McDonnell, J. J., Jarvis, W. T. (2012). The role of bedrock groundwater in rainfall–runoff response at hillslope and catchment scales. Journal of Hydrology, 450, 117-133.

Mosquera, G., Segura, C., Crespo, P. (2018). Flow Partitioning Modelling Using High-Resolution Isotopic and Electrical Conductivity Data. Water, 10(7), 904.

Fischer, B. M. C., van Meerveld, H.J., Seibert, J. (2017) Spatial variability in the isotopic composition of rainfall in a small headwater catchment and its effect on hydrograph separation. Journal of Hydrology, 547, 755-769.

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