**Hydrology and
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Discussions

Interactive comment on "Sampling frequency trade-offs in the assessment of mean transit times of tropical montane catchment waters under semi-steady-state conditions" *by* E. Timbe et al.

Anonymous Referee #2

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The paper "Sampling frequency trade-offs in the assessment of mean transit times of tropical montane catchment waters under semi-steady-state conditions" investigates the effects of different sampling intervals on Transit Time Distribution (TTD) estimates. Using weekly stream and soil water isotope samples together with event samples of precipitation, data is aggregated to different temporal resolutions for 2 scenarios: first, precipitation and stream/soil water is aggregated to the same temporal resolution; second, stream/soil water is kept at weekly resolution while only precipitation is temporally aggregated. Based on NSE values and uncertainties estimated by the GLUE method differences in TTDs and parameter results were evaluated and the conclusion drawn

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that the temporal sampling resolution can add uncertainty to TTD estimates. Finer resolutions were more similar, while coarser resolutions (>1 month) differed to a greater extent.

General Comments

1- A major part of the paper is redundant information when compared to the publication of almost the same authors: "Understanding uncertainties when inferring mean transit times of water trough tracer-based lumped-parameter models in Andean tropical montane cloud forest catchments", published 24 April 2014 in HESS. On closer inspection, the same (not quite; see Specific Comments) data set was used. It is only natural that one data set can suffice to conduct research into many different topics. However, when major parts of this submission can be deleted without loss of unique information as they are already sufficiently and well presented in Timbe et al. [2014] (the Sections: Study area, Hydrometric measurements, Sampling scheme and isotopic analyses, Isotopic gradient of rainfall, Lumped parameter equation to infer mean transit times of water, Transit time distributions of water, Model performance; additionally, part of Results and Discussions), it starts to feel uncomfortable. Comparison to Timbe et al. [2014] make Table 1, Table 2, Figure 1, Figure 2, parts of Figure 8 redundant, while parts of Table 3 can be reconstructed by Table 4 in Timbe et al. [2014]. More detailed comparison is given in the Specific Comments.

2- The Discussion section "4.2 Comparison of distribution functions" deals to a great part with a comparison of TTD models, their differences and their possible physical interpretation. Starting from the beginning of this subsection, only 7 lines out of 56 mention sampling frequencies, and that is in a subordinate clause, until this section returns on-topic. This is not and should not be the aim of the paper. In fact, this was a goal of Timbe et al. [2014], quote "(ii) characterization of the dominant TTD functions". However, the conclusions for dominant TTD even change in this submission when compared to Timbe et al. [2014], keeping in mind that it is the same data and this is nowhere discussed (see Specific comments).

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3- One of the messages of the paper is that coarser data sets (starting with monthly resolution) perform more different than higher resolutions (e.g., daily). I am mostly familiar with studies using weekly data, not so much monthly data. The authors could discuss this issue more with examples of studies that use monthly tracer data and are affected by this. 4- Some sentences are really long and convoluted and the use of comma interrupt the reading flow in places where it is unnecessary, making the paper not as easy to read as it could be.

I have read and thought about this paper several times and compared it to Timbe et al. [2014]. Although both studies investigate unique topics (sampling frequency vs. uncertainties), it is my opinion that they could have been combined into one paper given the amount of overlap. This theoretical paper would have dealt with sampling frequencies. It could have used all TTD models that were used in Timbe et al. [2014] and checked their performance based on the quality rules detailed in Timbe et al. [2014]. Although the topic of sampling frequency is very interesting and of scientific relevance, based on these considerations I recommend to reject this submission for publication in HESS for reasons of self-plagiarism and scientific discrepancies between the two papers using the same data set (with just a few months in-between these two papers). Should the editor decide otherwise, I hope that my detailed explanation of changes that need to be done are helpful to the authors in improving this manuscript.

Specific Comments

Comparison of this paper to Timbe et al. [2014] and comments on Figures and Tables. Left-hand side figure number is this paper, right-hand side figure number is Timbe et al. [2014]:

Figure 1 with Figure 1: Inevitably the same study site is shown. This is no big deal and only natural to occur. The amount of overlap regarding the rest of the paper however makes this figure problematic, as the Timbe et al. [2014] version shows more detail and could have easily been used in a composite paper.

Figure 2 with Figure 2: These are almost identical. Striking here is a shift in time series. While the submitted paper is from 9/1/10 to 9/1/12, Timbe et al. [2014] is from 8/1/10 to 8/1/12. This is very confusing and goes hand in hand with discrepancies in both texts about the start of the sampling campaign and discrepancies found in similar tables in both papers. Why is the time series shifted?

Figure 3: There is no similar figure in Timbe et al. [2014], but this figure could be removed without drastic loss of information. It is nice to see how the aggregated time series look, but not absolutely necessary to understand this paper.

Figure 8 with Figure 8: the left panels of soil water site C in this paper is the same as Timbe et al. [2014] panels a and b, with the Timbe et al. [2014] version showing more information due to more TTD models. This is another example of how the composite paper could have looked and that redundant results are presented.

Table 1 with Table 1: This is a direct copy, with slight changes in formatting, with Timbe et al. [2014] showing more information.

Table 2 with Table 2: Not a direct copy, but very similar with lot of redundant and even mismatching information. While rainfall collection started in one case in August 2010 and was done by Collector, in the other case it started in October 2010 and was done Manually. This coincides with the text information given in the respective papers. Main Rivers and Tributaries collection dates also differ. Which is true? Why is there even a difference? Where is the exact difference between Manually and Collector for rainfall samples? This goes hand-in-hand with the question of the 1-month shifted time series and does raise some concerns.

Table 3 with Table 4: These tables deal both with soil water simulation results using the LPM model. As the time series is only shifted a month, they must be comparable, which they are to a certain extent. The Sampling frequency of "1 week" in this paper is the same setup as in Timbe et al. [2014]. In most cases, the soil sites A to F of this paper are almost (?) the mean value of the split-up version in Timbe et al. [2014], e.g.,

for parameter tau: B = 4.53 (4.50), C = 3.79 (3.93), D = 6.29 (6.20), E = 6.11 (6.60), F = 4.49 (4.87). Only soil site A is different with 5.69 (4.56). Why is A suddenly different in mean transit time using weekly data?

Equations 3, 4 and 5 with Table 3: only natural that the TTD model equations are the same, but still, if you combined these to one paper, it would have saved space again.

Discrepancies of TTD models for soil and stream sites: Why is it that in Timbe et al. [2014] in the Conclusion it says, quote "In this sense, using the best predictions from models like LPM for soil waters and EPM for surface and spring waters yielded a more reliable range of MTT inferences through lowering the uncertainty associated in the predictions of certain models." while in this paper is reads, quote, "In this regard, for soil water the gamma distribution not only provides the highest goodness of fit but also more realistic and meaningful predictions; although for dampened isotopic signals (i.e stream waters) a model preference is still not clear, [. . .]". This directly contradicts each other, and they were the findings of the same authors, in the same catchment, using the same data set and methods. This is not discussed in the paper.

Further, in this paper in the Discussion starting on p 12463 line 27: "For soil waters LPM yielded similar tau predictions than GM. However, a simple look at both distribution functions demonstrates that the gamma distribution function can provide more detailed information on how and when the tracer's signal increases/decreases and when the peak occurs". Yet, Timbe et al. [2014] prefers LPM for soil waters. This does not add up.

More specific comments Definition of "tau" changes: 12452, line 12: tau is given as tracer transit time (agreed). On 12455, line 7 it is "tracer tau", 12457 onwards it is subsequently used as "MTT of the tracer (tau)" in the results discussion (e.g. for GM the median tau).

p 12449, line 6: "The flashy reaction of the hydrograph [. . .]" where are those data coming from? From Timbe et al. [2014]? The source of the baseflow information

should be cited here.

p 12452: Equation 1 is not necessary in this paper, as no radioactive tracer is used. It would safe several lines of text if you only describe the equations you used.

p 12455: choosing of the second peak of the parameter that is not responsible for the MTT and writing that it will improve convergence of parameters needs a reference or written as assumption by the authors.

p 12460, line 16: the value of 0.71 from bimonthly data actually looks to be the furthest away from the daily value of 0.63, when considering all sampling solutions. Why do you write it is close to it?

p 12463, line 4: What are "mean conditions of soil water"?

p 12466, line 6-7: quote "For our study catchment, considering that NSE are high for all models and that TTD does not seem to influence their performance but greatly influences the predicted MTT, additional insights need be explored in order to unveil the correct TTD-function, as solely relying on model performances could lead to misleading results" Of course the TTD greatly influences the MTT, as the MTT is derived from the TTD. There is no more insight needed, as there are already results from Timbe et al. [2014]. If they are not valid anymore, you need to give a reason.

p 12467, line 3: What are micro-catchments?

p 12467, line 11-13: Sampling frequency cannot account for time-variable conditions, how should it?

Technical Corrections

I am no expert in this, but many comma seems more irritating for fluent reading than helpful:e.g. p 12460, line 8 "Results for particular sites, follow nearly [. . .] Similar, to the results [. . .]"

p 12445, line 19: environmental tracers are defined as stable water isotopes. The term

environmental tracers has appeared already often at this point, it would be better to put the definition there.

p 12450, line 23: "top of a 1.5 m standing pole".

p 12451, line 1: missing words, probably "were collected".

p 12452, line 5: "deducted" should be "deduced"

p 12453, line 1: "to describe a catchment's TTD"

p 12454, line 19: it is called Global Likelihood Uncertainty Estimation, not Analysis.

p 12454, line 21: "lower limit was established"

p 12454, line 26: the abbreviation BIAS is not defined.

p 12455, line 23: Eq. (1) should be Eq. (2)

p 12457 and ongoing: I would start the results description by describing the NSE values of the simulations first, to start with the quality of the simulation results and not directly with the behavior of the MTT.

p 12457, line 11: that daily rainfall data is referred to as "reference sampling resolution" should be declared in the methods section and not in results.

p 12457, line 14: use MTT instead of mean transit time

p 12458, line 18: "delta-tau showed/had extreme values"

p 12458, line 24: "accounted for"? maybe change to "found for"

p 12459, line 21: "slightly increased".

p 12459, line 22: "varied little"

p 12463, line 9: at "he" I would start a new sentence. Also change "he" to "they", as Birkel did not write his publication alone.

p 12465, line 29: "oversimplify the water flow"

p 12466, line 2: there is no MTT distribution function, but TTD.

p 12467, line 1: sentence is unclear with "to account the more reliable distribution function."

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12443, 2014.