

## ***Interactive comment on “Insights on the water mean transit time in a high-elevation tropical ecosystem” by G. M. Mosquera et al.***

**G. M. Mosquera et al.**

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Referee 2:

The manuscript “Insights on the water mean transit time in a high-elevation tropical ecosystem” by Mosquera et al. under review in Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2015-546, 2016 presents an attempt to investigate MTTs of a nested paramo catchment system in Ecuador with the purpose to tease out dominant controls on water transit time. The authors were able to identify relatively short transit times (< 1yr) compared to other environments in different climatic regions. The MTTs in their study site are mainly controlled by the catchment slope in relation to the dominant wetland soils. The experimentally derived dataset for this tropical ecosystem is unique and interesting to the HESS readership and beyond. The analysis is mostly sound and

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the paper generally well-written and structured. Having said that, the paper struggles in parts to clearly convey the main points in line with the objectives of the study and could be shortened. I am missing a discussion around arguments that the MTT is not a meaningful catchment descriptor and the recent tendency towards the recognition of the time-variant nature of transit times. I do think that there are merits in using the MTT to characterize catchment systems particularly considering the constraints and limitations working in tropical environments; it should, however, be more clearly argued. Furthermore, there are some model decisions that should be more clearly explained, which also likely leads to additional analysis strengthening the paper and its line of arguments. Nevertheless, I think this is nothing that cannot be fixed with a careful revision to improve clarity and focus of the paper and I therefore support publication of this paper with some revisions.

Reply: We appreciate Christian Birkel’s (R2) revision and his constructive suggestions to improve the scientific quality of the manuscript and look forward to publishing this work in HESS hoping to improve the understanding of hydrologic processes in tropical ecosystems. We agree that the paper could be shorter to focus its content in the objectives. We also acknowledge that some details of the modeling procedure deserve clarification along with a discussion about recent theoretical frameworks that explicitly incorporate time-variant transit times (please see response to R1). Our responses to R2 comments are outlined below.

Specific comments:

Abstract:

Line 21: I’m not sure if the paper is about streamwater MTT as you excluded high-flow events from the analysis. Reply: We agree, based on the discussion below, it is clear that MTT estimations correspond to baseflow MTTs. This will be specified throughout the manuscript.

Key words:

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Line 15: I suggest to simplify and reduce the key words to attract more online search results, e.g.: Ecohydrology, MTT, runoff generation, Andean paramo, Histosols, Ecuador.

Reply: We agree with the suggestion and will simplify and reduce the key words to the following: Ecohydrology, MTT, runoff generation, wet Andean páramo, high altitude tropical wetlands, Histosol, Ecuador

Introduction:

Page 3, Line 2: This is true for Latin America, but there are a few more studies in the tropics. You could even refer to Muñoz-Villers and McDonnell (2012) in this context.

Reply: We agree and will include these additional references: (Farrick and Branfireun, 2015; Muñoz-Villers et al., 2015) of studies conducted in the tropics.

Page 3, Line 17: I will come back to this point, but I think it's very likely that there's also a considerable near-surface runoff component as seen in other environments (you refer to Scotland and Sweden below) with organic rich wetland soils that remain saturated for much of the year. I, however, don't know the paper in review you cite here.

Reply: We see that this point can cause confusion. However, by "shallow subsurface flow" we refer to water moving in the first 30-40 cm within the soil matrix (i.e., "shallow organic horizon of the páramo soils located near the streams"). This will be clarified in the text. This is supported by water isotopic concentrations observed in the shallow organic horizon of the Histosol soils and stream waters in the study area. These observations and further discussion are presented in a manuscript currently under review in Hydrological Processes. We will provide the editor of this paper with a copy the referred manuscript to pass it along to the reviewers.

Page 3, Line 29: This isn't entirely true, I'm afraid, because the dominating runoff generation process based on various tracer studies is a rapid near-surface flow. The subsurface component is a deeper and slower groundwater flux. Therefore, the wetland contribution can be quantified very well in form of near-surface saturation overland

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flow.

Reply: We partially agree with this comment. The hydrologic functioning of this particular system takes place in the first 30-40 cm of the soils, here referred to as "shallow subsurface flow", and saturation excess overland flow rarely occurs in the Zhurucay basin (i.e., high flows occur less than 3% of the time at the study site, see figure 3 in Mosquera et al., 2015). Although it is true that "rapid near-surface flow" has been observed in other environments, it mostly refers to "near-surface saturation overland flow", and therefore, it does not apply to our system. We will clarify this point by changing the term "subsurface" by "shallow subsurface" in the revised version of the manuscript.

Page 4, Line 9: I think Broxton et al. (2009) worked in Arizona, USA. You could also specify the control you are referring to as in this case it was "aspect".

Reply: We appreciate the suggestion. We will incorporate this reference in the manuscript and specify the controls on MTT.

Study site:

Page 5, Line 8: This is an awkward sentence, please revise.

Reply: We agree and will revise and restructure the sentence.

Line 10: seasonality, primarily.

Reply: These suggestions will be incorporated.

Line 23: please, spell out INV.

Reply: INV is the name of the mining company (<http://www.invmetals.com/about/history/>). We will remark this in the manuscript.

Page 6, Line 4-5: Please, revise this sentence.

Reply: The sentences referring to soil distribution in the catchment will be restructured.

Line 27: Please, indicate model and make of the equipment.

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Reply: Model and make of the instrument (Schlumberger DI500) will be indicated.

Methods:

Page 9, Line 26: . . . is based. . .

Reply: We agree with this suggestion and will correct accordingly.

Line 27: I'm not sure I follow the second point.

Reply: This refers to input (recharge) function of the precipitation tracer composition to take into account recharge (i.e., volumetrically weighted isotopic composition) (McGuire and McDonnell, 2006). This will be clarified.

Page 10, Line 6: In this case, I suggest to consistently refer to a baseflow MTT and not streamwater MTT.

Reply: We agree. The following will be added: "As a result, estimations correspond to baseflow MTTs, hereafter simply referred to as MTT."

Page 11, Line 2-5: I fully agree that you seek to identify the best-performing and most parsimonious model. However, you don't really compare the models using a criterion for model selection (e.g., AIC, BIC or adjusted R<sup>2</sup>) that penalizes the number of parameters in combination with a goodness-of-fit measure. The MI criterion looks at how identifiable one parameter is, but not at the combined effect of more than one parameter used to calibrate the model.

Reply: We appreciate this comment. We applied the AIC metric for model selection to our results and will include the results from this analysis in the manuscript. This analysis indicates that the exponential model (EM) is indeed the best.

Page 11: How were models generated? Using a uniformly sampled Monte Carlo procedure?

Reply: This is correct. The fitting procedure included two steps for each model. 1)

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Initially 10,000 sets of parameter values were evaluated considering a wide range of parameter values sampled according to a uniform Monte Carlo procedure. The parameter ranges were wide. For instance the parameter range of the MTT of the EM model varied between 0 and 130 biweeks (5 yrs). 2) After the initial 10,000 runs, the range of the set of parameters that displayed relatively well identified were narrowed and the model was run again until 1,000 behavioral parameter sets were obtained (i.e., sets of parameters that yielded solutions corresponding to at least 95% of the highest KGE).

Line 16: mainly?

Reply: We appreciate the suggestion "Majorly" will be changed to "mainly".

Results:

Page 12, Line 12: Runoff coefficients show. . .

Reply: This sentence will be updated accordingly.

Page 13, Line 4-26: I'm not convinced by some of the statements present in this paragraph. For example, the best-fit gamma model compared to the best-fit exponential model does show a quite significant increase in performance (from 0.63 to 0.75) that can justify the use of one additional fitting parameter. On the other hand, a third fitting parameter resulted in an increased performance of only 0.01. The poorest model seems to be the DM with a best-fit of KGE=0.5. Based on this, one could qualitatively reject the DM and TPLR models as suitable models compared to the EM and GM. However, the decision between the EM and GM models should be informed by a model selection criterion such as the AIC (see comment above) that evaluates the combined effect of the parameters on model performance.

Reply: We completely agree and appreciate the suggestion. As indicated above we have now conducted an AIC evaluation that confirms our model selection. The EM model is indeed the one that yields the lowest AIC score.

Page 14, Line 12: Please, revise this sentence.

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Reply: Thank for the suggestion, the sentence will be revised and corrected.

Page 14: I think that large parts here could be moved into the discussion or simply be deleted as later sections pick up on these issues. This would allow to shorten the m/s focussing on presenting the key results and later discussion in the light of the wider literature.

Reply: We agree with this suggestion. We will trim down irrelevant text in this section. In particular, we will consider removing or attaching the section regarding the pdf and cdf TTD curves as supplementary material.

Page 15, Line 26: just use MTT

Reply: This will be corrected accordingly.

Page 16, Line 5-12: Please, separate this very long sentence into smaller parts.

Reply: We appreciate the suggestion. This sentence will be split into three.

Discussion:

Page 17, Line 26: more depleted?

Reply: You are correct, this will be updated.

Page 18, Line 1: I think it would be better to indicate that baseflow MTT was analysed.

Reply: We agree and will change this accordingly.

Line 3: identifiability?

Reply: We agree.

Line 20: Was TMCF previously defined?

Reply: Thank you catching this. A definition will be included.

Line 30: remain.

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Reply: We agree.

Page 19, Line 10: You somehow have to convince me that this actually is subsurface stormflow. I haven't seen the in review paper you mention in this context and all the evidence you show tells me that the dominating runoff generation mechanism is near-surface saturation overland flow due to little mixing with deeper soil horizons, short MTTs, etc.

Reply: Based on the characterization of the weekly isotopic composition of stream and soil waters conducted over a two-years period, it is evident that the isotopic composition of the shallow organic horizon of the Histosol soils consistently matches that of the streams, and that precipitation has essentially no influence in the streamflow isotopic composition (Mosquera et al., 2016). As such, even "subsurface stormflow" appears to inappropriately describe the system's functioning, as water is preferentially delivered from the shallow 30-40 cm of the organic horizon of the Histosols to the streams regardless of the precipitation dynamics. Moreover, saturation excess overland flow rarely occurs at the study site (Mosquera et al., 2015). Therefore, we consider that "shallow subsurface flow" is indeed the appropriate term to define the delivery of water from the Histosols situated at the bottom of the slopes to the streams. We will include a clearer explanation of this.

Line 20: You previously said up to 2.2 years in this context.

Reply: We referred to two different studies conducted in central Mexico. Muñoz-Villers and McDonnell (2012) reported MTTs of three years and recently Muñoz-Villers et al. (2015) reported MTTs ranging between 1.2-2.2 years. Therefore, this statement in the manuscript is correct.

Page 20, Line 6: I think Hrachowitz et al. (2009) argued with saturation overland flow.

Reply: Hrachowitz et al. (2009) actually reported that at the Lord Arch catchment runoff generation shows a flashy catchment response "dominated by runoff processes in the

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upper soil horizons.” That is, in the 40 cm depth peaty soils, overlaying the mineral horizons,

Line 11: solutes.

Reply: Agree.

Page 22, Line 1: explain.

Reply: Agree.

Line 8: Please, revise this sentence.

Reply: We agree. This sentence will be reworded.

Line 14: Isn't this simply the slope?

Reply: Yes, you are correct. We will change it accordingly.

Line 24: I find the “regulation capacity” is coming a bit out of nowhere. What exactly do you mean by this? Is it in the sense of resilience or simply that the turn-over is quick and what goes in comes out with little delay?

Reply: We acknowledged how this can lead to confusion. We will clarify this in the text. Basically the páramo is an ecosystem recognized for its high discharge regulation capacity (i.e., páramo generates runoff year-round regardless of variability in precipitation inputs to the system). This characteristic is essential to the sustainability of human activities of downstream populations. However, little is known about the factors driving this regulation capacity. The results from this study provide information that improves our understanding of catchment functioning by identifying some of these drivers. That is, the interplay between soil storage and topography. We will explain the regulation capacity notion of the ecosystem in the introduction section of the manuscript.

Page 23, Line 2: It's the first time that you mention that SOF wasn't previously observed in the study catchment. This information needs to come earlier. I also think this whole

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paragraph can be shortened towards the key messages presented at the very end.

Reply: We agree. We will add information regarding SOF earlier in the manuscript. In addition we will trim this paragraph to reduce its length.

Line 29: Please, revise this sentence.

Reply: We agree, we will reword the sentence.

Tables:

Shouldn't the current Table 2 come before you present the models (Table 1)?

Reply: We appreciate this suggestion and completely agree.

Current Table 1: I'm a bit confused about some decisions concerning the choice of initial parameter intervals. Why was the upper limit of tau set at 200 biweeks? This makes 2800days and over 7 yrs of TT, something stable isotopes aren't able to detect anyways (Stewart et al., 2010). Further, why was the lower limit of beta (GM) set to 0.5? In the case of low TT this could be well below 0.5 and on a global scale the average resulted to be at around 0.5 (Godsey et al., 2009). With the current lower limit in place you potentially miss suitable parameters that would also result in lower MTTs compared to current best-fit results; an argument you used to reject the GM. Also, it seems odd to me that you don't report the parameter interval for beta as this is the parameter you calibrate. The MTT (tau) is only the result of beta\*alpha.

Reply: We really appreciate this comment. We used the MTT parameter ranges suggested by Timbe et al., (2014). However, we recognize that these authors had a different objective in their study and that it is reasonable that we constrain our parameter values range for MTT up to 5 years (130 weeks) (McGuire and McDonnell, 2006). As such, we will run all models again for all catchments, and statistics and figures will be updated accordingly. Regarding the parameter in the GM, we believe R2 refers to the alpha parameter. The alpha parameter lower limit was originally set up at 0.01, and the 0.5 value was just mistakenly reported in the table. We apologize for the confusion this

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caused. We will correct the lower limit value in the table and also report the parameter range considered for beta.

Table 5: Similar issue here with the GM. I suggest to report the parameters alpha and beta.

Reply: Same as above. Beta parameter will be reported in the figure.

Table 7: R2-values of 0.62 did not result significant? However, there's a relationship with flow characteristics particularly for the extremes and the runoff coefficient does seem to explain some of the spatial variability among catchments.

Reply: We appreciate this comment. The mentioned relations are not statistically significant at a 95% confidence level. Results are as follows: Runoff coefficient: R2 0.62 p-value: 0.39 Q99: R2 -0.42 p-value 0.24 Q10: R2 -0.61 p-value 0.12 Q5: R2 -0.62 p-value 0.11 However, we agree that there is a relation between baseflow MTTs and low flows that deserves to be considered. Thus, p-values will be included in table 7 and relation with low flows will be discussed accordingly.

Figures:

Figure 2: What's the purpose of the streamflow inlet box? Could you not just show a log-scale to emphasize the low flow periods? Those event samples do show quite a bit of response to rainfall. What's the effect of pooling these out? Quite a bit shorter MTTs? Please, consider adjusting the different EC sampling period for comparison purposes.

Reply: 1) Streamflow inlet box: The purpose of the streamflow inlet box is to emphasize the response of low flows to rainfall inputs during the less humid periods. The box indicates flashy response even during these periods. We therefore still believe that the non-log-scale representation of the hydrograph in combination with the inlet box provides the best impression of the observed dynamics. 2) Event samples: The model runs reported were originally conducted once these referred event samples were

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pooled out from the streamflow isotopic composition time series. 3) EC sampling period: Sampling period for EC will be adjusted to hydrometric and isotopic data sampling period.

Figure 5: Please, clarify if sampling was started below alpha = 0.5 (GM) contrary to the information from Table 1. Again, I suggest to present the parameters alpha and beta.

Reply: We apologize for the confusion. The lower limit of the alpha parameter was originally set up at a value of 0.01. This was updated in Table 2. We now present both alpha and beta parameters as suggested, together with the MTT.

Figure 6: Is EPM missing in the right panel?

Reply: No, it is not. It just plots behind the EM curves in both panels. A note will be added to the caption to explain this.

Figure 8: If the MTT is normalized shouldn't it be unitless?

Reply: You are correct. We will change accordingly in figures 8 and 9.

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