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 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.

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.

Key words:

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.

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.
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.
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 flow.
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".

Study site:
Page 5, Line 8: This is an awkward sentence, please revise.
Line 10: seasonality, primarily.
Line 23: please, spell out INV.
Page 6, Line 4-5: Please, revise this sentence.
Line 27: Please, indicate model and make of the equipment.

Methods:
Page 9, Line 26: …is based…
Line 27: I'm not sure I follow the second point.
Page 10, Line 6: In this case, I suggest to consistently refer to a baseflow MTT and not streamwater 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 R2) 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.

Page 11: How were models generated? Using a uniformly sampled Monte Carlo procedure?
Line 16: mainly?

Results:
Page 12, Line 12: Runoff coefficients show…
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.
Page 14, Line 12: Please, revise this sentence.
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.
Page 15, Line 26: just use MTT
Page 16, Line 5-12: Please, separate this very long sentence into smaller parts.

Discussion:
Page 17, Line 26: more depleted?
Page 18, Line 1: I think it would be better to indicate that baseflow MTT was analysed.
Line 3: identifiability?
Line 20: Was TMCF previously defined?
Line 30: remain.
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.

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

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

Line 11: solutes.

Page 22, Line 1: explain.

Line 8: Please, revise this sentence.

Line 14: Isn't this simply the slope?

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?

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

Line 29: Please, revise this sentence.

Tables:

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

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.

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

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.

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.

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.

Figure 6: Is EPM missing in the right panel?

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

References I used:

Godsey, S. E., Aas, W., Clair, T. A., de Wit, H. A., Fernandez, I. J., Kahl, J. S., Malcolm, I. A., Neal, C., Neal, M., Nelson, S. J., Norton, S. A., Palucis, M. C., Skjelkvåle, B. L., Soulsby, C., Tetzlaff, D. and Kirchner, J. W. (2010), Generality of fractal 1/f scaling in catchment tracer time series, and its implications for catchment travel time distributions. Hydrol. Process., 24: 1660–1671. doi: 10.1002/hyp.7677.

Stewart MK, Morgenstern U, McDonnell JJ. 2010. Truncation of stream residence time: How the use of stable isotopes has skewed our concept of streamwater age and origin. *Hydrological Processes* **24**: 1646–1659.