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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS

The paper attempts to explore transit time distributions (TTD) in a high-elevation tropical ecosystem by using a detailed hydrologic and isotopic record from eight nested catchments located in southern Ecuador. Although the data are extremely interesting and unique in quality and location, the transit time analysis is performed through a method (the lumped convolution approach) which is likely to include an aggregation bias, especially for systems with a high degree of heterogeneity and non-stationarity (see the recent papers by Kirchner, [2016a,b]). In simple terms, even if the transfer function approach allows a fair simulation of the measured isotopic signal, the system mean transit time is not necessarily realistic, due to the structural uncertainties in the quantification of the older water components. This emerges in Figures 4 and 6, where different TTD (with different MTT) result in similar model performances. Moreover, the

paper ignores the recent advances in hydrologic transport and TTD (see the list of suggested literature), which are now widespread within the hydrologic community and have clarified the concept of TTD in the light of non-stationarity. The manuscript is clear, well written and easy to follow, but the methods pose some serious concern on the paper's conclusions.

DETAILED COMMENTS

Page 7, line 18: the authors say that kinetic fractionation by evaporation can be neglected, however looking at Figure 3 it seems that the majority of stream water samples plot below the LMWL. How can this behavior be explained?

P. 9, l. 3: the variable tau in Eq. (1) and (2) is not the mean transit time. It is just the dummy variable in the integral, which spans the transit time domain [0, +inf].

P. 10, l. 22-26: I did not get why the model is run twice to get the behavioral set of parameters.

P. 10, l. 28: the MI index seems to be very arbitrary depending on the choice of the prior parameter distribution. Segura et al., [2012] provide a partial explanation for their choice of the prior, which is here missing.

P. 13, l. 27: the terminology "MTT probability density function" seems to refer to the pdf of MTT obtained from the posterior parameter distribution.

P. 14, l. 1-13: this is to me a clear example of the indetermination of the MTT. Different parameterizations of the TTD are able to provide good, similar simulations of the isotopic signal, but result in rather different MTT. While it is reasonable to choose a model because its parameters are more constrained in the simulation of a specific target, this does not allow to extrapolate that its MTT is the "right" one.

P. 15, l. 21: what is meant by "completely" recovered? Is there a threshold (e.g. 99%) on the recovered mass?

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