Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-61-RC3, 2018

Hydrology and Earth System Sciences Discussions
Open Access

Interactive comment on "Land use alters dominant water sources and flow paths in tropical montane catchments in East Africa" *by* Suzanne R. Jacobs et al.

Anonymous Referee #3

Received and published: 19 March 2018

The study of Jacobs et al. used weekly data of the isotopic and chemical composition of streamwater, precipitation and other end members in three nested catchments with different land use in Kenya, Africa, to analyze how differences in land use may affect streamflow generation. To test this, the authors used end-member mixing analysis to estimate the relative contributions of the end-members to streamflow, as well as use a convolution approach to calculate the mean transit times of streamwater at catchments with different land use. While I consider the data set and the research question relevant for the readers of HESS, there are some parts that should be addressed before publication.

Printer-friendly version

Discussion paper

The mean transit time (MTT) estimates based on a data set covering ca. 1.5 years are likely to be highly uncertain. This is evident, for instance, in the similar numbers of NSE, RMSE and Bias for the streamwater and soil water samples at the sites SHA and TF (Table 3): While streamwater was sampled weekly at these sites (n>100), MTT estimates were similarly uncertain for streamwater as for soil water - from which only a small number of samples was collected (n<17)! Thus, based on the model performance criteria presented in the manuscript, I would not strictly believe the values obtained for streamwater either. Although the authors elaborate on the shortcomings of their data set with regard to estimate MTTs (Sect. 4.3), they do not consider using an alternative approach such as the young-water fraction framework (Kirchner, 2016a, b). This framework uses the seasonal cycle amplitudes of streamwater and precipitation amplitudes to estimate the fraction of water younger than ca. 3 months. Thus, with the data set presented by Jacobs et al., such an analysis might result in estimates of the young-water fractions of streamwater that are more robust than the MTTs. (Using the soil water samples from the sites NF and OUT might also reveal some interesting results, however, the data from the sites SHA and TTP are clearly too incomplete for such an analysis.)

In the catchment SHA, the samples from a wetland (WL, n=4) and the shallow well (WE.b, n=2) comprised two important end-members in the 3-component mixing analysis, whereas no wetlands or shallow wells were sampled in the other two catchments. Thus, I question the comparison made between the three sub-catchments: the relative contribution of precipitation at a site is inevitably linked to the contributions of the other two end members (all components must add up to 1), and therefore the precipitation contributions of NF and TTP cannot simply be compared with the precipitation contributions of SHA.

In general, I find the presentation of the solute concentrations of the different end members and streamwater insufficient - although this data set builds the foundation for the whole study. In the box plot (Figure 2) it is very difficult to distinguish between the dif-

Printer-friendly version

Discussion paper

ferent sites (vertical gridlines would help here) and end-members (distinction between the different end members would be impossible in a BW print). I suggest that the authors elaborate more on the data set, incl. uncertainties and times of sampling. Are the times of sampling representative for the flow regime at the sites or were the samples only collected during low-flow conditions? A presentation of the data similar to Figure 4 might be useful for this.

Abstract:

- The numbers presented in p1, L27-29 for the average relative contributions of springs and wetlands to streamwater are confusing: wetlands were only analyzed for one catchment (SHA), and in the Abstract it appears as if wetlands and springs were considered equivalent end members. In addition, I don't understand how the numbers presented in p1, L29-31 confirm that "... catchment hydrology is strongly influenced by land use, which could have serious consequences for water-related ecosystem services, such as provision of clean water.". Do the authors compare agricultural (i.e., de-forested) catchments to an un-altered forested catchment (i.e., baseline scenario)? If this is the case, then the results should be presented within such a framework.

Introduction:

- The different sub-sections of the introduction should be linked better. For instance, paragraphs 1 and 2 present two very different topics (tropical montane catchments and stable water isotopes, respectively), which have to be put into a common context, otherwise the reader is lost.

- The authors hypothesize that (a) streamwater in the natural forest sites is (on average) older than streamwater in agricultural catchments (smallholder agriculture, tee and tree plantations); (b) precipitation comprises a larger fraction of streamflow in the agricultural catchments than in the naturally forested catchment; and (c) that seasonality in rainfall causes temporal variability of these streamwater sources throughout the year. The formulations of the working hypotheses (a) and (b) are somewhat redundant: when streamflow at site A contains more precipitation (i.e., ”new” water) relative to another site B, we should expect the mean transit time of Site A to be shorter. Thus, hypothesis (a) results from hypothesis (b). Regarding hypothesis (c), I don’t understand how accepting/rejecting this statement adds to the conclusions of this study. The authors discuss hypothesis (c) only briefly later in the manuscript (p11, L21-23), which makes me wonder why it is stated so prominently in the Introduction?

Methods:

- P3,L30: What are the areal fractions of different land-use types in the main catchment (OUT)? This information would also be required to elaborate on the authors’ statement on p13, L18-20: “One could also expect that, since OUT is a mixture of the three land use types dominating the sub-catchments, the MTT should be similar to or an average of the estimated MTTs of the sub-catchments.”. This statement would only be true if the three sub-catchments are representative for the areal fractions of land use in the main catchment. - 2.3 Sampling and laboratory analysis: What are the instruments’ measurement precision and accuracy? Especially in the case of Li, the measured concentrations (« 1ug/L) might be highly uncertain for precipitation and throughfall. Results: - 3.2 Isotopic composition: “There was no significant effect of elevation on δ18O values of the precipitation samples, but precipitation samples collected at higher altitude (SHA-PC) were generally more depleted than those collected at lower altitudes (NF-PC, TTP-PC and OUT-PC).“. This sentence is confusing, please reformulate.

- Figure 6 and analysis of Figure 6: Some of the relative contributions are highly uncertain, however, I miss a proper uncertainty analysis here. Although the authors discuss various sources of uncertainty in Sect. 4.2., a quantitative uncertainty analysis is still missing. At least, showing the error bars in Figure 6 would be helpful to interpret the results with more caution (i.e., Could the variability of the end members be an artefact of uncertainty in the EMMA?, p11 L21-23)). In addition, the Abstract, the authors present the average contributions without any uncertainty measures, which might be misleading. Discussion: - 4.2 Dominant water sources: Based on another study in the

NF catchment (Jacobs et al., in review) the authors conclude that in the NF catchment precipitation reaches the stream network via shallow sub-surface flow. Short residence times in the shallow subsurface thus result in dilution effects in streamwater. However, for the TTP catchment, the authors claim that "...surface runoff could have a different chemical signature than precipitation..." (p11, L13), which somewhat contradicts their previous statement in L3: "Therefore, if event water, i.e. precipitation or throughfall, is only in contact with the soil for a short time (e.g. several hours), the chemical composition of the water that enters the stream might be comparable to the composition of precipitation or throughfall.". Please clarify this.

Minor comments: P8, L13: Where these evaporated samples used in the analysis? Please clarify. P9, L28: "... has been observed elsewhere as well." – Where exactly? Are these sites comparable to the sites of this study? P12, L15: An alternative method to sample soil water would be suction lysimeters. P14, L23: "Due to the similar soils..."

References: Kirchner, J. W.: Aggregation in environmental systems-Part 1: Seasonal tracer cycles quantify young water fractions, but not mean transit times, in spatially heterogeneous catchments, Hydrol. Earth Syst. Sci., 20, 279-297, 10.5194/hess-20-279-2016, 2016a.

Kirchner, J. W.: Aggregation in environmental systems-Part 2: Catchment mean transit times and young water fractions under hydrologic nonstationarity, Hydrol. Earth Syst. Sci., 20, 299-328, 10.5194/hess-20-299-2016, 2016b.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-61, 2018.