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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.

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We would like to thank Reviewer #1 for his valuable feedback on our manuscript. The reviewer identified several major issues with the methodology used. Here we would like to reply to the major comments made by Reviewer #1:

1) The MTT methodological explanation is adequate (if some citation of GLUE development papers is included) but it fails to describe how a (400?) year-long 18O input function has been obtained to feed the lumped models when the rainfall sampling period was just 75 weeks long.

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Reply: Bracketed values shown in the third column of table 2, i.e., tau = [1-400], correspond to the range of values that the MTT parameter could take for solving the convolution integral. By mistake, the units for these values are missing. This will be corrected during the revision of the manuscript. The equations were fed with weekly data, which was the interval of the sampling campaign. Therefore, the value of 400, means 400 weeks (=7.7 yr), which is a long enough period to cover the maximum possible values that the MTT could take for solving the convolution function. According to literature, it is appropriate to use stables isotopes of water for MTT estimations of up to 4 or 5 years. Regarding to the 'limited' length of sampling period (75 weeks for the isotopic signal of rainfall), used to feed the lumped models, we hypothesize a constant interannual recharge of the aquifers. We acknowledge that, ideally, it is advisable that the length of the sampled period is at least comparable to (or longer than) the length of the estimated MTT. However, for remote tropical montane catchments, data is generally scarce because of limited funding, harsh meteorological conditions and challenging accessibility. On the other hand, an advantage of tropical areas compared to temperate zones, is the low interannual and intra-annual variability in terms of meteorological characteristics like temperature and precipitation. Based upon this reasoning, preliminary insights of the rainfall-runoff characteristics of a tropical catchment could be derived from assuming that the seasonality of the isotopic signal of one year could resemble that of another. In this sense, it is reasonable to artificially extend a short input time series (rainfall) through repeating the available sampled time series in a loop. For our case, the input isotope time series were repeated 20 times. Repeating the input time series in a loop is a common practice where input data is limited, and not only for studies of tropical montane forest (Muñoz-Villers and McDonnell, 2012; Timbe et al., 2014; Hrachowitz et al., 2010 and 2011).

2) It has been shown that MTT determinations using seasonal variations of tracer signals (such as the 18O one) cannot provide acceptable results longer than a few months in stream (mixed) waters due to the strong non-linearity of the driving function (Kirchner, 2016).

Reply: The approach presented by Kirchner (2016) is a valuable contribution to the study of the rainfall-runoff behavior of natural heterogeneous systems, which, by the way, need to be studied assuming non-linearity. As pointed out in the referred publication, the estimation of MTT through tracer cycles and methods like the lumped convolution approach, as used in our manuscript, should be limited to homogeneous catchments for which steady state conditions apply. We acknowledge that natural systems are implicitly heterogeneous, however their degree of heterogeneity could be highly variable. How heterogeneous or homogenous does a catchment have to be to prevent, or allow, the use of traditional approaches like lumped parameter models? Some ideas are posed in the referred work: Figure 4 in Kirchner (2016) shows two contrasting outflow isotope signals: a highly damped signal and another whose amplitude closely resembles the one of rainfall. This means that before using the traditional approach in the study of nested catchments, we should first check if the amplitudes of the sampled sites are comparable. For instance, if two elements (sub-catchments) of a nested catchment have contrasting amplitudes, then we should be aware that the combination of these two outflows will provide an unrealistic amplitude and therefore an unrealistic MTT. On the other hand, if amplitudes of isotope signals of outflows are similar, it will be a preliminary indication of homogeneity (i.e., little to moderate heterogeneity), and therefore traditional steady state approaches could be applied. The standard deviation (σ) is a proxy of the amplitude of the isotopic signal (e.g., Garvelmann et al., 2017). For our data, σ values for the observed input functions (i.e., rainfall) for every catchment are 2.59‰ 2.73‰ and 2.54‰ for NF, SHA and TTP, respectively. On the other hand, σ for outflows (for the same period, according to the observed data) are 0.10‰ 0.11‰ and 0.07‰ (for NF-RV, SHA-RV, and TTP-RV, respectively). Then, an intercomparison between the amplitudes of every river outflow can be easily performed through a simple fourth proportional calculation: 12.4%, 11,4% and 10.3%, (for NF, SHA and TTP, respectively). The latter values correspond to the amplitude of the outflows compared to the original amplitude of the rainfall, expressed in percentages. The similarity between the amplitudes of the three analyzed catchments is an indicative of homogeneous characteristics, meaning that they could be characterized by a single transit-time distribution. Furthermore, the young water fractions (YWF) of NF-RV, SHA-RV and TTP-RV, calculated as a ratio between the amplitude of the outflow (stream water) and input signal (rainfall) are also similar: 3.72%, 4.08%, and 2.67%. These little portions of YWF, show that analyzed stream waters correspond to baseflow dominated catchments in which steady state conditions could apply. In this respect we should have in mind that due to the highly damped signal of the analyzed outflows, no discrimination was performed between samples taken during baseflow or high-flow conditions (i.e. all 75 stream water samples for each catchment were included in the analysis). Another way to check if homogeneity was correctly assumed could be to check if just one type of transit time distribution function provides the best results for all the analyzed sub-catchments and/or the best parameters of that single function are similar or comparable among catchments. For our case, the gamma model provided the best fitting efficiencies, and the model parameters were also similar for two out of the three analyzed catchments (the results for TTP-RV were discarded because of its low fitting efficiency, NSE=0.05).

3) For such damped tracer signals in the stream waters and low model efficiencies, much larger MTT uncertainties should be obtained, showing results coherent with point 2. My opinion is that the small uncertainties obtained are an artefact due to the way the behavioral models have been selected in the GLUE exercise. Accepting only parameter sets with efficiency just 5% lower than the optimal one might be appropriate for high efficiency values, but not in the case of such low efficiency values because the range of behavioral parameters becomes too narrow. Some GLUE published works dealing with large uncertainties sensibly used all parameter sets with positive efficiencies. Alternatively, all the parameter sets with such low efficiencies might be rejected as a way to resolve that the method is inappropriate.

Reply: We agree with the comment of the reviewer 1: if the adjustments of the model are low, a range of 5% below the best solution, becomes narrow. For all our analyzed stream waters, the model efficiencies were low, mainly due to the highly damped isotopic signal (see point 2). The calculated uncertainties are just a way of comparing the degree of identifiability of the model parameters, among the sub-catchments under study. These results (i.e., ranges of solutions) are not meant for comparison with ranges of uncertainty of other studies with substantially higher NSE. However, since scatterplots of behavioral solutions are not presented in the paper, the presented ranges are useful for the reader to know if the models converge to a unique best solution or not (i.e. whether behavioral solutions tend to a peak or if they have a flat shape). In the section 4.3 of our paper, we acknowledge that the fitting efficiency was too low for TTP-RV (NSE = 0.05) and therefore their associated results should not be considered. In the same section we also acknowledge that for these cases: “Better predictions could be obtained by using more appropriate tracers for estimating transit times of several years to decades, such as tritium (3H) (Cartwright et al., 2017). A longer sampling period of at least 4 years would also improve the reliability of the mean transit time estimates (McGuire and McDonnell, 2006)...” In this respect, in the revised version of the paper we will emphasize that due low fitting efficiencies of stream waters, results for SHA-RV and NF-RV should also be taken with care, and only as an indicative that MTT of several-years older. In the case of stream waters, I suggest to remove the proposed MTT determinations, unless the above points are adequately answered. The authors may reasonably continue using the clear damping of the tracer signal in the stream waters as an indicator of several-year old waters, and even the differences in the temporal variability of the tracer signals might be used to indirectly rank the waters MTTs. In the case of soil mobile waters, I suggest the application of some analysis of the significance of MTT differences found, using the MTTs likelihood distributions provided by the GLUE exercise. As requested by Reviewer 1, after clarifying some aspects (detailed previously in this reply), which will be explicitly included in the revised version of the paper, we believe that it will be important to keep the MTT determinations since these preliminary results will provide a base knowledge in this remote study area for which no previous data was available. We welcome the suggestion

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of Reviewer 1 for soil mobile waters: to include a sort of MTTs likelihood distribution function analysis. In this respect, cumulative density functions (CDF) (together with their respective analysis and discussion) for the three analyzed soil water sites and for both models: EPM and GM, considering the range of associated uncertainty, will be included in the revised version of the paper.

4) The third but also relevant issue refers to the End Member Mixing Analysis (EMMA) for the Small Holder Agriculture (SHA) stream waters. The use of the well SHA-WE.b as end member representative of groundwater chemistry is not reasonable. One well in the headwaters with solute concentrations very different from those in other nine wells may represent either a different water source or some pollution effect, but it is not sensible to hypothesize that it can be a relevant source for stream water when its chemistry is very local as it is not transmitted to the other well waters. If well understood, the use this end member with very low contributions as representative of groundwater is depicted in Figure 7 (b), although this is inconsistent with some text in the conclusions: "A second, different groundwater source was identified in the smallholder agriculture catchment, which was an important end member during baseflow"

Reply: We understand the concern of the reviewer about the use of a single 'outlier' to explain stream water chemistry and mixing of different end members in a catchment and, ideally, we would have identified another end member which would fit the end member model better than SHA-WE.b. Nevertheless, of the sampled end members, SHA-WE.b is the only end member that can explain the stream water chemistry of samples taken during the dry season (Figure 5 in manuscript). As mentioned in the discussion (P. 11 L. 29-31), it is likely that end members are missing and a further effort should be made to develop more appropriate and less uncertain end member mixing models. Considering that this is the first effort to characterize hydrological flow paths and water provenance in this tropical montane area, we think that the use of SHA-WE.b is reasonable to present preliminary findings, given the available data. With regard to the question whether SHA-WE.b is a polluted well or represents a different groundwater source, the chemical composition (high concentrations of Si, Li, K, Na and Rb) of SHA-WE.b resemble the elements that could be expected in groundwater based on the geology of the area. We consider both the wetland SHA-WL and SHA-WE.a/SHA-WE.b groundwater sources. Since SHA-WL is an important contributing groundwater end member, especially during the wet seasons, referring to SHA-WE.b in the conclusion as a second groundwater source seems reasonable. However, we indeed need to revise the sentence referred to by the reviewer, as the use of the word ‘important’ overestimates the potential role of this end member in stream flow generation.

Aside from these major issues, other points indicated by Reviewer #1, such as the title of the manuscript, the use of the expression ‘soil water’ and the detailed comments will be addressed in the revised version of the manuscript.

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