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Discussions

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 #1

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The commented manuscript presents an exercise to analyse dominant sources and transit times for stream and soil mobile waters in a tropical montane catchment in East Africa, subject to diverse land uses.

General comments:

The subject of the manuscript may be of interest for HESS readers, it represents a relevant work volume and is well presented, but there are several formal and methodological issues that deserve a major revision of the manuscript before being acceptable for publication. The first issue is in the title of the paper itself. It is very assertive while the results of the work, taking into account the associated uncertainties, are much less

convincing. So, I suggest to change the title or just to put it into an interrogative form.

The second but main issue is respect to how Mean Transit Times (MTTs) have been obtained for stream waters. The several aspects of this issue are the following ones:

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.

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).

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 behavioural 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 behavioural 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.

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.

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. The analysis done can be shown as a test, but it cannot be taken as representative because groundwater contribution becomes underestimated and the other components overestimated. 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”

Another more formal issue is the use of the ‘soil water’ expression to identify the samples of mobile waters sampled at different soil depths. In the current water isotope literature, ‘soil water’ refers to the total (bulk) water contained in the soil, including mobile and immobile waters. In the methods section it is clearly justified that just mobile water was sampled, but in the abstract, figures and conclusions, some adjective such as ‘mobile’ or ‘free’ should be added to ‘soil water’ in order to avoid any misunderstanding.

Detailed comments:

As most of the paper should be rewritten only major remarks not included before are made

- Page 3, line 21: some hypothesis on how rain water reaches the stream should be added

- Page 7, line 8: Nash & Sutcliffe (1970)

- Page 8, line 11: GLUE was first described in Beven & Binley (1992)

- Page 3, line 21: “Ten shallow wells (nine named SHA-WE.a and one SHA-WE.b)...”

- Page 8, lines 10 and 15: the units for the slopes are not correct.

- Page8, line 15; page 10 line 9: this slope value seems too small looking to the graphs.

- Page8, line 31: the contribution of precipitation to SHA stream waters is overestimated due to the role of SHA-WE.b commented above

References

Beven, K., & Binley, A. (1992). The future of distributed models: model calibration and uncertainty prediction. Hydrological processes, 6(3), 279-298.

Kirchner, J. W. (2016). Aggregation in environmental systems–Part 1: Seasonal tracer cycles quantify young water fractions, but not mean transit times, in spatially heterogeneous catchments. Hydrology and Earth System Sciences, 20(1), 279.

Nash, J. E., & Sutcliffe, J. V. (1970). River flow forecasting through conceptual models part IâĂŤA discussion of principles. Journal of hydrology, 10(3), 282-290.

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