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Interactive comment on "Assessment of surface water resources availability using catchment modeling and the results of tracer studies in the meso-scale Migina Catchment, Rwanda" *by* O. Munyaneza et al.

Anonymous Referee #2

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This paper analyses the controls governing the rainfall-runoff transformation in a mesoscale African catchment by means of a consolidated modelling approach. The paper results are intended to be possibly used as practical tools in water resources management in Rwanda. This approach is interesting and the paper deserves to be published. However, I have several concerns about how the manuscript was structured and organized, and on what topics should have been stressed in order to make it a clearer and with a more impact. In my opinion, the main points that are weak and that should be really improved are the following: i) The introduction is not as effective as

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it could be in presenting the main reasons for which the study was undertaken. The main aim of the work is bit vague and should be specified, clearly stating the main research questions. Moreover, I understand that the ultimate purpose is to use the results as tools for decision-making but a) it is not described how the results could be useful for this purpose; and b) to have a bigger impact on the hydrological community, the paper should be also present some scientific problems or, at least, highlight what is still missing in the comprehension of hydrological behaviour of this catchment and of similar catchments in Rwanda and/or in other similar conditions (in other words, the novelty of the paper). From this stems the necessity to report previous literature works that have dealt with similar issues. ii) Discussion is weak and falls too short. Sometimes, interpretation of results in not even provided (see Specific comments). A more comprehensive analysis encompassing a) the overall obtained results; b) the comparison of such results with previous ones in the same study area and in other catchments where the same approach have been used; and c) a discussion around the research questions that should be included in the introduction is needed. iii) The results part is also a bit weak (there are basically two Tables and two Figures). Some extra analyses, suggested below, could perhaps make the results more robust. iv) There is some redundancy in the description of some methods (e.g., model structure, Nash-Sutcliffe index . . .) but other parts that could ease the interpretation of the results (e.g., description of the calibrated model parameters) are missing (see Specific comments).

In the end, I suggest a major revision of this manuscript before its publication in HESS.

Specific comments: P376. Skip lines 1-6 and start the abstract with "In the present study" adding later the name and the size of the catchment

P377, L21-22. Why does this call for modelling studies? Please, explain better.

P378, L22. In the whole paragraph it is not clear what these methods are and why are important. Please, specify. P379, L7-8. Please explain better what "Rwanda has been moving from centralized to decentralized water resources management" means.

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P379, L7-21. I suggest to move all this part to the Introduction, after L5 at P378.

P380, L7-13. Again, I suggest to move all this part to the Introduction.

P381, L24. What are "manual rain gauges"? Storage gauges? If not, and if home-made, do they have standard size? Please, specify.

P382, L3. Which method(s) has/have been used to make discharge measurements? How many measurements? In which flow range? When? Please, specify.

P383, L5-6. Already mentioned, remove.

P383, L10-14. Not important for the paper purposes, remove.

P383, L17-18. Already mentioned, remove.

P383, L21. This is not clear, please explain better.

P385, L3-4. Already mentioned, remove.

P386, L17-19. It is mentioned later, please remove.

P387, L1-12. Nash-Sutcliff index was introduced in 1970 and, since then, used in many modelling works (i.e., it is very well known). Therefore, I suggest to remove all this part.

P387, L14-P388, L4. I think that this part fits better the introduction, in a possible new subsection where what is known about the hydrological response of the catchment from previous studies is reported.

P388, L11-21. This part is interesting but the authors should a) first, present the model parameters in the Method section; and b) extend the discussion and comment more in detail the differences/similarities among the various sub-basins.

P388, L23. Here, and also in P389, L11 and L12 and P392, L29. Add "n" and "p-value"

P389, L13-14. This should be mentioned earlier, in the Method section, when present-ing the model parameters.

P389, L14-17.This is interesting but where are the data? How do the authors explain this disagreement between weak correlation and what is observed (but not shown)?

P390, L3. Any hypothesis for this relatively low value of NS?

P391, L2-4. Perhaps, although there are only 5 points, it's worth trying a correlation between imperviousness and direct runoff.

P391, L10-onward. Here a new Figure could be added, where the rainfall and stream-flow, with its component, could be shown to facilitate the comparison between the tracer-based results and the model results.

P391, L12-15. This part should be moved to the introduction where previous results for the catchment could be reported.

P391, L27-P392, L2. Idem.

P400, Table 2. I suggest to remove the initial values of the parameters. If included, a discussion on the comparison of the initial and calibrated parameters should be also added to the text.

P403-404. I suggest to swap Fig. 1 and Fig. 2, since the current Fig. 2 seems more "introductive" to me than the current Fig. Of course, the corresponding part in the text should be modified accordingly.

P405. I don't understand the usefulness of Fig. 3. This behaviour can just be described in the text without actually showing it. I suggest to remove it.

P407. Fig. 5. The subplots are too small (at least, the labels cannot be well read). There is a "3" that should be superscript in the left y-axis of the last subplot. More importantly, there seem to be some shifts between the observed and the simulated streamflow that should be better highlighted (maybe at a more detailed scale) and discussed in the text. Add the NS index to each subplot.

P408. I suggest to change the Fig. into a stacked bar plot so that it's more immediate

to compare the results obtained by the two methods for the same event.

Minor comments: P376, L13-14. Remove "each represented by one of the five observed streamflow gauges."

P376, L13-16. Add "index" after "Model Efficiency"

P378, L21. Remove "related"

P380, L16. "vary" should be "varies"

P380, L24. add "ESRI" before "ARCGIS". Also at P383, L16.

P380, L27-28. I suggest to use one digit for the description of the size of the subcatchments (here and also in Table 1).

P381, L22. "meteorological" is better than "climatic"

P382, L2. "Mini-divers" is the commercial name. If I'm correct, they are pressure transducers. Please, correct and specify.

P389, L2. Remove ",".

P389, L24. Remove "Eq. 3".

P390, L23. Remove the second "is"

P392, L22. Correct the English.

P394, L8. Add an "s" to "value".

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 15375, 2013.