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Hydrology and Earth System Sciences
Discussions

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.

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Author's Response for Referee #2

Ref. No.: hessd-15375-2013 Title: Assessment of surface water resources availability using catchment modeling and the results of tracer studies in the meso-scale Migina Catchment, Rwanda

We appreciated very much the feedback of the second referee. All specific corrections

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suggested have been addressed in the revised manuscript. In the following, the most important changes are detailed point by point.

Major issues

Major issue i): Referee #2: The introduction is not as effective as it could be in presenting the main reasons for which the study was undertaken. Authors: The whole introduction has been rewritten in the revised manuscript.

Referee #2: 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; Authors: The main aim has been revised as well as the whole introduction. The main aim was reformulated in the revised manuscript as follows: "The main objective of this study is to analyse spatial variation of runoff generation characteristics of the Migina catchment using a semi-distributed hydrological model. The model is expected to assist as a tool for water resources planning and decision making processes in this catchment". The conclusion shows how the results of the tool will be useful for decision making on P394, L2-7 as follow: "Lumping the entire Migina catchment would lead to missing important aspects of some of the sub-catchments and, subsequently, potentially misinforming the planning and decision making processes. Depending on the purpose of the assessment and the intended use of the information to be generated, individual units at an appropriate scale may require particular attentions even in very small catchments".

Referee #2: 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.

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Authors: We agree with the reviewer and more recent literature has been added in the revised manuscript (e.g. Neary et al. 2004; Fleming and Neary, 2004; Cunderlik and Simonovic, 2005; De Laat, 2006; Chu and Steinman, 2009; Munyaneza et al. 2012b). The novelty of the paper was shown in the revised manuscript: "This piece of work went beyond the standard calibration of the model to the total flow to verify estimated values of one runoff components, i.e. baseflow. Baseflow contribution estimates cannot be validated using the standard method (comparison with records). This paper called for a new approach with which the baseflow results by the Rainfall-Runoff model were verified using the results of tracer investigations. This is not a classical model validation (like a split sample test as recommended by Klemes, 1986), however, it provided further insights into the model behavior and the model performance".

Major issue ii): Referee #2: Discussion is weak and falls too short. Sometimes, interpretation of results in not even provided (see Specific comments). Authors: We agree with the reviewer's comments and the discussion was improved and this could be seen through how authors addressed in the revised version the specific comments raised by both referees #1 and #2.

Referee #2: A more comprehensive analysis encompassing a) the overall obtained results; Authors: We have addressed this through the specific comments below and the revised manuscript was improved accordingly.

Referee #2: 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; Authors: We have made this correction in the revised manuscript and more recent literature on the same study area (e.g. SHER, 2003, Nahayo et al., 2010, van den Berg and Bolt, 2010, Munyaneza et al., 2010, 2012a,b) has been added in the revised manuscript, especially in the introduction and discussion parts.

Referee #2: and c) a discussion around the research questions that should be included in the introduction is needed. Authors: We have made this correction in the revised

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manuscript.

Major issue iii): Referee #2: 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. Authors: We have improved the results in the revised manuscript and is now richer due to the consideration of suggestions and comments from referee #1.

Major issue iv): Referee #2: 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). Authors: We have addressed these comments through the specific comments below; the description of the calibrated model parameters was improved in the revised manuscript.

Specific comments

Comment 1: Referee #2: P376. Skip lines 1-6 and start the abstract with “In the present study” adding later the name and the size of the catchment. Authors: We have made this correction in the revised manuscript.

Comment 2: Referee #2: P377, L21-22. Why does this call for modelling studies? Please, explain better. Authors: We have addressed this in the next sentence as follows: "Rainfall-runoff models have been widely used in hydrology over the last century for a number of applications, and play an important role in optimal planning and management of water resources in catchments (e.g. Pilgrim et al., 1988, O’Loughlin et al., 1996)".

Comment 3: Referee #2: P378, L22. In the whole paragraph it is not clear what these methods are and why are important. Please, specify. Authors: We agree with the reviewer’s comments and the whole paragraph was revised in the new version. HEC-HMS applications in African regions were reviewed as well.

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Comment 4: P379, L7-8. Please explain better what “Rwanda has been moving from centralized to decentralized water resources management” means. Authors: We have made this correction in the revised manuscript by adding the following sentence. "This has been done in line with addressing the goal number 7 (to ensure environmental sustainability) of the Millennium Development Goals (MDGs) by elaborating Rwanda Vision 2020, EDPRS I (2007-2012) and EDPRS II (2013-2018)".

Comment 5: Referee #2: P379, L7-21. I suggest to move all this part to the Introduction, after L5 at P378. Authors: We have made this correction in the revised manuscript.

Comment 6: Referee #2: P380, L7-13. Again, I suggest to move all this part to the Introduction. Authors: We have made this correction in the revised manuscript.

Comment 7: Referee #2: P381, L24. What are “manual rain gauges”? Storage gauges? If not, and if homemade, do they have standard size? Please, specify. Authors: This sentence was removed in the revised manuscript as the next sentence was enough to give the message. "Rainfall data from 12 stations were only used in this study, given that the rainfall data....".

Comment 8: Referee #2: P382, L3. Which method(s) has/have been used to make discharge measurements? How many measurements? In which flow range? When? Please, specify.

Authors: We have revised the paragraph in the revised manuscript. Flow range, number of measurements as well as method used were all added and shown as follows. "Rating curves were established using discharge measurements at different periods from May 2009 to June 2011 (Eq. 1). The recorded water levels were converted into discharge values using rating curves ($r^2 = 0.88$, $n = 25$ at Rwabuye station; $r^2 = 0.96$, $n = 25$ at Akagera station; $r^2 = 0.94$, $n = 24$ at Kansi station; $r^2 = 0.80$, $n = 28$ at Mukura station; and $r^2 = 0.97$, $n = 18$ at Migina station). Note that n represents the number of discharge measurements. The applied rating curve is presented for instance by De Laat and Savenije (2002 and De Laat (2006):

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(1) where Q is the discharge in m3 s-1, H is the water level in the river in m, H0 is the gauge reading at zero level, and a and b are constants. The value of H0 is determined by trial and error while the values a and b are found by a plot on logarithmic paper and fit of a straight line or by a least square fit using the measured data".

Comment 9: Referee #2: P383, L5-6. Already mentioned, remove. Authors: We have made this correction in the revised manuscript.

Comment 10: Referee #2: P383, L10-14. Not important for the paper purposes, remove. Authors: We have made this correction in the revised manuscript.

Comment 11: Referee #2: P383, L17-18. Already mentioned, remove. Authors: You are right. We have removed the corresponding text in the revised manuscript.

Comment 12: Referee #2: P383, L21. In addition, the HEC-GeoHMS created background map files and basin model files, which were later used by HEC-HMS to develop a hydrologic model. This is not clear, please explain better. Authors: This comment can be seen in the revised manuscript P380, L21-L25 as follows: The catchment boundaries were delineated from the Digital Elevation Model (DEM) map obtained from the USGS website with a resolution of 90 m using GIS tools and sub-catchment areas were generated automatically by HEC-GeoHMS 5.0 with ESRI ArcGIS 10.0. The catchment was subdivided into 5 sub-catchments as shown in Figure 1.

Comment 13: Referee #2: P385, L3-4. Already mentioned, remove. Authors: We have made this correction in the revised manuscript.

Comment 14: Referee #2: P386, L17-19. It is mentioned later, please remove. Authors: You are right. We have removed the corresponding text in the revised manuscript.

Comment 15: Referee #2: 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. Authors: We agree with the reviewer that the Nash-

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Sutcliffe index is very well known and we have shortened the paper here. We just showed the used formula and how it was used in the paper.

Comment 16: Referee #2: 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. Authors: We have made this correction in the revised manuscript and the whole section was revised.

Comment 17: Referee #2: 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. Authors: The model parameters were presented on P385, L18-21, in the method section of the revised manuscript on as flows: "In the model parameterization process, each hydrologic unit was supplied with initial conditions and parameter values based on the requirements of the different computation methods as discussed in the Section 3.3. Initial parameter values were selected based on previous (published) works where available, otherwise default values from the manual were applied. Table 2 shows the five computation methods: canopy, surface, loss, transform; and baseflow, the type of parameters used for each method, and values attributed to each parameter in the modeling process (initial and calibrated)".

Comment 18: Referee #2: P388, L23. Here, and also in P389, L11 and L12 and P392, L29. Add “n” and “p-value” Authors: We have made this correction in the revised manuscript.

Comment 19: Referee #2: P389, L13-14. This should be mentioned earlier, in the Method section, when presenting the model parameters. Authors: We have made this correction in the method section on P384, L17 of the revised manuscript.

Comment 20: Referee #2: 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)? Authors: We have shown data in Tables 1 and 2 in the

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revised manuscript for the clarification.

Comment 21: Referee #2: P390, L3. Any hypothesis for this relatively low value of NS=0.38 at Munyazi sub-catchment? Authors: We have added the following sentence in the revised manuscript. "This low value of NS observed at Munyazi sub-catchment could not be fully explained and more research should be done".

Comment 22: Referee #2: P391, L2-4. Perhaps, although there are only 5 points, it's worth trying a correlation between imperviousness and direct runoff. Authors: We have made this correction in the revised manuscript.

Comment 23: Referee #2: P391, L10-onward. Here a new Figure could be added, where the rainfall and streamflow, with its component, could be shown to facilitate the comparison between the tracer-based results and the model results. Authors: Figure 6 is showing the comparison between the tracer-based results and the model results. The Figure was improved as suggested in the below comment 30.

Comment 24: Referee #2: P391, L12-15. This part should be moved to the introduction where previous results for the catchment could be reported. Authors: We have made this correction in the revised manuscript.

Comment 25: Referee #2: P391, L27-P392, L2. Idem. Authors: We have made this correction in the revised manuscript.

Comment 26: Referee #2: 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. Authors: We have removed the initial values in the revised manuscript.

Comment 27: Referee #2: 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. Authors: We have made this correction in the revised manuscript.

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Comment 28: Referee #2: 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. Authors: We have removed Fig. 3 in the revised manuscript.

Comment 29: Referee #2: 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. Authors: We have improved Fig. 5 in the revised manuscript and NS index was added to each subplot as suggested. The text was improved as well and discussed on P390, L14, as follows: "The main reason that our model simulates high and low recession of baseflow (at Cyihene-Kansi and Migina outlet, respectively) after a storm event may be linked to the inflexibility of the model structure. The results could have been improved by using flexible model structure, e.g. FLEX-Topo (Savenije, 2010; Fenicia et al., 2008a,b and 2010; Gao et al., 2013). Savenije (2010) demonstrated that FLEX-Topo model allows the groundwater time scales to be lumped and determined by manual calibration on the recession curve. A more flexible model structure would allow to design in particular the runoff generation module (different runoff response functions) based on the obtained process understanding and physiographic characteristics and dominant landscape elements such as riparian zones, hillslopes and hilltops.".

Comment 30: Referee #2: 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." Authors: We have addressed this comment in the revised manuscript for the comment suggested in to the abstract on P376, L13-14, and the figure on P408 was improved as shown in Figure 4.

Comment 31: Referee #2: P376, L13-16. Add "index" after "Model Efficiency" Authors: We have made this correction in the revised manuscript.

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Comment 32: Referee #2: P378, L21. Remove “related” Authors: We have removed this in the revised manuscript.

Comment 33: Referee #2: P380, L16. “vary” should be “varies” Authors: We have made this correction in the revised manuscript.

Comment 34: Referee #2: P380, L24. add “ESRI” before “ARCGIS”. Also at P383, L16. Authors: We have made this correction in the revised manuscript.

Comment 35: Referee #2: P380, L27-28. I suggest to use one digit for the description of the size of the subcatchments (here and also in Table 1). Authors: We have made this correction in the revised manuscript.

Comment 36: Referee #2: P381, L22. “meteorological” is better than “climatic” Authors: You are right. We have changed this in the revised manuscript.

Comment 37: Referee #2: P382, L2. “Mini-divers” is the commercial name. If I’m correct, they are pressure transducers. Please, correct and specify. Authors: We have corrected in the same way in the revised manuscript and the type of mini-diver used in the paper was shown. Pressure transducers (mini-diver; DI501).

Comment 38: Referee #2: P389, L2. Remove “,”. Authors: For better clarification we have added “,” after “that” on P389, L1 of the revised version.

Comment 39: Referee #2: P389, L24. Remove “Eq. 3”. Authors: We have made this correction in the revised manuscript.

Comment 40: Referee #2: P390, L23. Remove the second “is” Authors: We have deleted the whole paragraph due to the fact that evaporation was made clear in Table 4 of the revised manuscript.

Comment 41: Referee #2: P392, L22. Correct the English. Authors: We have corrected the English (rephrased the sentence) in the revised manuscript as follows. "However, nothing fully explains the higher base flow contribution to the total runoff

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compared with Munyazi and Mukura sub-catchments, except that all the three sub-catchments present different hydrological elements (e.g. topography, shape of river channel)". The authors understanding to the catchment is that they have wider valley floods in the downstream part of the catchment and more groundwater contributions in these parts/subcatchments compared to upstream catchments with more narrow valley floors and less shallow groundwater storage.

Comment 42: Referee #2: P394, L8. Add an “s” to “value”. Authors: We have deleted the paragraph in the revised manuscript because our used HEC-HMS model does not allow entering soil texture/properties during the model set-up as explained in comment 3 above.

On behalf of the authors, Omar Munyaneza, Kigali, Rwanda June 2014

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

Author's Response for Referee #1

Ref. No.: **hessd-15375-2013**

Title: **Assessment of surface water resources availability using catchment modeling and the results of tracer studies in the meso-scale Migina Catchment, Rwanda**

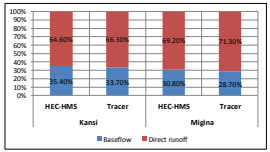

Figure 4 Comparison of flow components results using HEC-HMS model (current study) and hydrochemical tracer method (obtained from Munyaneza et al., 2012a) for two investigated events in the rainy season in 2010 and 2011, using flow data collected at Kansi and Migina flow stations.

On behalf of the authors,
Omar Munyaneza, Kigali, Rwanda
June 2014

Fig. 1.

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