

Interactive comment on “Hydrological impact of rainwater harvesting in the Modder river basin of central South Africa” by W. A. Welderufael et al.

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Comment 1: It is not clear in the text how this study is different from previous work (Mwenge Kahinda et al. 2009 etc.). Please clarify how this study adds to the present understanding of IRWH potential in the area. Response: This has been addressed in the revised manuscript

Comment 2: Ensure to present the most important statistical results in the abstract so that the reader directly understands the main point of the article. Response: Comment accepted and statistical results included in the abstract

Comment 3: Please ensure to justify various claims with proper statistical justification

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(see attachment). Response: Comments accepted and adjustments have been made in the manuscript based on the attached supplement

Comment 4: If I understand right the calibration period is only 1 year, and there is no validation period. As explained in the attachment, the authors need to justify this, and an independent validation period is needed. Response: We obtained six years (2002–2006) observed daily mean flow data for the gauging station C5H056 which recorded the streamflow for the 419 km² sub-catchment inside C52A catchment. But, as we mentioned in the manuscript, we found that only the year 2002 data to be reliable for our purpose. So, we used this data to optimize or calibrate those parameters which were found most sensitive (ranked 1 to 7) to the streamflow. This has improved the prediction of the streamflow during 2002. A remarkable improvement was achieved graphically which is also presented in statistical test in the text when the year 2002 daily stream flow was simulated by using the calibrated values. Of course, it would have been nice to have a validation test to get more confidence on the model in predicting the real situation. Unfortunately, we were not able to do the validation test due to the unreliability in the data. Therefore, by considering the suitability of SWAT in predicting the streamflow of ungauged streams, the result that was obtained without doubt yielded at least a relative comparison among the water balances of the different land use scenarios.

Comment 5: Please provide a clearer and more detailed definition and justification of scenario setups. Response: Comment accepted and addressed in the manuscript

Comment 6: Parameter non-uniqueness is a commonly observed issue when calibrating SWAT (see e.g. Andersson et al. 2009 in HESS, www.hydrol-earth-systsci.net/13/2329/2009/). Can you elaborate on the possible effect of alternative equally likely parameterizations on the scenario results? Response: The doubts raised by the referee in relation to parameter non-uniqueness have been properly addressed in the revised manuscript.

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Comment 7: The methods section needs to be revised considerably. Response: Comment accepted and revisions have been made on the methodology section of the manuscript based on referee's suggestion.

Comment 8: Proper use of units and addition of references Response: Comments accepted and revisions have been made in the manuscript

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