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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impacts of agricultural intensification through upscaling of suitable rainwater harvesting technologies in the upper Ewaso Ng'iro North basin, Kenya" *by* J. K. Mutiga et al.

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The paper describes a study that analyses the effect of changes in land use/land cover on the water resources in the upper Ewaso Ngiro North basin, Kenya The topic of the study is interesting and brought with some references to literature, although most of the citations are relatively old and a recent state of the art is missing. The main problem however is that study lacks a proper scientific methodology, analysis and reporting. The following issues are seen as most problematic: (1) Calibration procedure: a SWAT model has been used for the simulation of different land use/land cover, but a proper

calibration of the model parameters has not been demonstrated. Note that for land use change studies, it is not enough to have a good fit to the observations in the flow, but that the parameters that are associated to different land uses need to be well identified. In addition to this, the parameter values are not listed and discussed. More worrying is that on page 2486, line 10, it is stated that the parameters are recalibrated for the different land use scenarios. The normal procedure is that parameters for the land uses are kept constant and that only the land use is changed in the models. No details on the procedure are reported for these recalibrations (parameter values, period of calibration etc). (2) Land use scenarios. An important issue is the previously described calibration procedure. Another problem is that the results do not mention the period of simulation. With the high variability in climate, one can not just compare a certain period of time against another period of time. Also the length of the simulation period should be long enough to account for the climate variability. Also, the same period should be used in order to be able to compare the hydrological components as in table 3. Since the rainfall inputs are different, the simulations are obviously not done for the same periods. (3) HWR scenario. It is absolutely unclear how the RWH scenario has been implemented, as there is no standard procedure for it in SWAT. How were the CN and evapotranspiration changed? How were the weather input data obtained for the period 2003 – 2015??? How could the authors justify results such as on 2488, line 5-10 stating that the groundwater contributions were going down due to RWH? Somehow, RWH should increase the infiltration (runoff water can be used for irrigation, hence increasing the green water storage with the potential to have more penetration to the shallow aquifer). These findings also contradict to the figures of table 3! Small remarks: (1) Model set-up should be described after data description. It should also include how the HRU's were defined – what were the thresholds used for land use/soil classes (this is a very important issue in land use change studies). (2) Better describe the processing of the land use maps. Which methodology? How were these validated? (3) What soil data have been used? What was the source for the DEM data? (4) Why was the Penman-Monteith method used for evapotranspiration? (5) 2480- line 13: RWH

abbreviation has been used before describing it (line 27) (6) 2481 line 7: what is ment with “natural resource base”? (7) 2484 line 20: daily performance is giving, while the rest of the paper states that performances are measured on monthly basis (8) 2486, line 18: what is SSA? (9) 2491, line 25: 60% probability of occurrence of below average rainfall – this is not really a surprising statement. Also better describe the agricultural drought. (10) Literature should also be updated with more recent publications.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2477, 2011.