Comments: "Hydro-Climatic Modelling of an Ungauged Basin in Kumasi, Ghana"

General comments:

This paper deals with a very important topic on water balance processes and their changes over time according to climate change. This study focuses on the Owabi watershed in Ghana which provides about 20% of drinking water demand of the Kumasi metropolis. The objective of the study is the application of the SWAT model to simulate the streamflow and the water balance of the watershed for the present period (1980-2015) and under future RCP8.5 scenario (2020-2050).

Although the ideas are very interesting and the manuscript well structured, I think this article is not yet mature and still requires work before publication. I have two major concerns:

1) My first general observation concerns the methodology part:

First, in the study site description, the authors should specify that the study site is located in the Sudano-Sahelian region under tropical semi-arid climate. This information allows understanding the systems involved in the region. On the other hand, the watershed is not really indicated in Figure 1. There is just a green rectangular delimitation, which corresponds to the Owabi Water Works Forest Reserve and not to the watershed. When I calculated the study site area, I find a surface, which is about five times larger than the value used in the manuscript (approximately 65 km² based on MNT, SRTM). This value corresponds to what is found in the recent literature (Akoto and Abankwa, 2014) even though actually Frimpong (2011) also gives an area of 13 km². May I feel that this area corresponds to the forest zone? With a clear delimitation of the watershed and a good scale, the question can be solved.

For input data, the authors used MODIS data for the land use map. These data have a wide spatial resolution given the size of the study site. Maybe that higher resolution satellite images (example of Sentinel-2 freely available on the study site) could be used to make a supervised classification and could be compared with MODIS results to take into account the associated errors. In addition, in Figure 2, which represents the watershed, different legend items do not appear on the map. I suggest that authors make several maps to allow the reader to visualize the study site but also to identify the different steps necessary for QSWAT to create the input data. For example: (a) MNT, (b) delineation and discretization of the watershed, (c) soil map, (d) vegetation map and (e) HRUs map. In the same way, a correspondence between soil units and QSWAT look up table is necessary.

Regarding meteorological data, the authors have used ERA-Interim to fill the gaps. Studying the correlation between ERA-Interim data and observed data when available will be interesting to quantify a possible bias. What happened for the gaps before 1989 (start date of ERA-Interim data)? Do you use ERA-40? If this is the case, it will be important to explain because ERA-40 is worse than ERA-Interim and could lead to systematic bias (see for example Mooney et al., 2011). Maybe this could explain the systematically underestimation of surface runoff in Figure 9 on the first simulation years. Please indicate the number of days missing for the different variables and for each year. In any case, I think that the choice to use these data can be justified but must be criticized and uncertainties should be calculated.

I understand the difficulty to obtain discharge data in West Africa but I think that the methodology used in this manuscript is too simplistic. The authors say that "In the decade-long review of the prediction in ungauged basins, it has been revealed that regionalization and other genetic networks can be used for stream-flow determination", but do not give any reference to support this remarks or to detail the methodology used. If I understand the approach to calculate the “estimated discharge”, the discharge was substituted by multiplying the daily precipitation data by a constant runoff coefficient (15%) over time. I think that this methodology is not appropriate for several reason: (a) the runoff coefficient, especially in West Africa, varies over time according to the increase of population and land use change for example but also during the year with the growth of herbaceous; (b) Processes in this region appears at a fine temporal scale (lesser than daily, hourly), so the use of daily precipitation to predict estimated discharge and the use of the same daily precipitation to simulate the runoff with SWAT is not a scientific research approach. I recommend some articles, which develop indirect methods to derive “estimated discharge” over West Africa thanks to reservoirs (see for example Gal et al. 2016, Rodrigues et al., 2013; Sawunyama et al.2006 or Liebe et al., 2005).

2) My second observation is about the novelty of this work and the expected results.

The summary and introduction of this article highlight the increase in human pressure on its environment and the consequences on water budgets. A simulation study (calibration-validation of the model) is then expected, followed by an evaluation of the impact of land use and climate changes on surface runoff. However, the results presented in this manuscript, relate simulation for a single climatic projection (RCP8.5) with the same land cover and soil maps as for the present (if I understand correctly). The conclusions drawn from this study seem to me a little too strong given the methodology used. Testing different climatic and landscape scenarios (hypothetical mixes) will help to consolidate the conclusions of this study as it has been done for the past by Forkuo and Frimpong (2012) for example. The same trend could be used to test a possible land use change for the future.

In page 14, the authors talk about the decrease of evapotranspiration and the increase of surface runoff in the future and gives as explanation that “this is expected because of the high deforestation rate that leaves the land surface bare to aid this process”. If the land cover map have been changed, I would agree with the authors’ arguments but this is not the case in this study, so I do not understand explanations and results advanced here! If the temperature is expected to increase, evapotranspiration will although increase but $ET_p < ET$ in Table 7. These results remain unclear for me. A scenario section in the methodology part will be useful to explain the approach.

Another point is about the average surface runoff for the present (0.073 m^3/s) and for the future periods (0.279 m^3/s) given in the manuscript. The Owabi reservoir has an average storage capacity of 2 600 000 m^3 (Akoto and Abankwa, 2014; Ghana Hydro-Database, 2017) so 412 days approximately are necessary for filling it in the present against 108 days in the future. According to Maoulidi (2010), the Owabi reservoir provides about 13 600 m^3/day for freshwater. This value is larger than the values given by the authors (6 307 m^3/day for present). The water supply given by the authors would therefore not cover the water demand.

There are too many questions and approximations in these results. Moreover, there is no explanation, no references to prove or argument the assumptions made and the consequences. I particularly regret the absence of a critical discussion of the results and limitations for the different work’s steps.

Specific and technical comments:

Figures 1 and 4 are too small and not readable.

Figure 2 should be modified based on previous comments. The delimitation of the reservoir could help the reader.

Figure 5 is not clear and the scale is too larger for some variable. Graphs (with their own legend) for each variable would be better rather than points.

For all Figures, please specify the variable name as it has been done in Figure 5.

Table 1: Please, specify the data resolution.

Table 6: Please, specify the units.

Page 2, Line 11: “management”.

Page 3, Line 19: The version of SWAT should be specified. SWAT2012 operates with an hourly, daily, monthly or yearly time step and not only with a daily time step.

Page 4: There is an error in the equation.

Page 6, Line 7: The wind speed is at 10 meters, the authors should note the equation that allowed them to have the wind speed at 2m (variable requested at the input of the model)

Page 8, Line 8: Specify the reason to choose 5 years of warm up and not 10 or 1.

Page 8, Line 9: “Penman-Monteith”

Page 8, Line 11: “Precipitation distribution was homogeneous within the watershed”. The explanations are not enough. It is a very important assumption in West Africa that needs to be discussed.

Page 8, Line 13: Is it a runoff by exceeding the soil infiltration capacity (Hortonian runoff)? If yes, please specify in the study site section.

Page 10: Pay attention during the calibration procedure. When modeling future projections, the parameters to be calibrated cannot be variable over time.

Page 10, Line 11: Please, specify the unit.

References: This section lacks many volume numbers, page numbers, editor or doi. Be careful.

Throughout the manuscript, be sure to give references.

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