The authors would like to thank our anonymous reviewer for his critical evaluation of the manuscript and comments. They have been very insightful.

General Comments

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).

Response: The Owabi catchment indeed extends to about 69 km^2 area. The aim of the study was to model the hydrology of the 13 km^2 forest sub-catchment only. But after careful consideration of your comments, we have decided to incorporate the larger catchment area into the modelling. The input maps for delineation of watershed, soil, vegetation and HRU maps will be added in the revised manuscript. ERA-INTERIM reanalysis data was used and the timeseries begins from 1979. Although comparison of daily minimum and maximum temperatures (2000 – 2004) from the Owabi station and ECMWF data showed a consistent agreement (R = 0.6), currently, we have used the nearest weather station data from Kumasi Airport to fill in the missing temperature gaps. The correlation analysis between ERA-INTERIM and the observed data can only be performed for the temperature values since there exists no station data for solar radiation, wind and dewpoint temperature. Hence the Hargreaves model has been considered for estimation of evapotranspiration. The number of missing days of data would be incorporated in the revised manuscript. A considerable amount of effort is being put into obtaining discharge data from Owabi, which has not been very fruitful yet. Therefore, applying the spatial proximity setting, streamflow from the nearest gauging station (River Offin) is being used to run the model for the Owabi catchment. A new landuse data from the European Space Agency, which characterises the landuse change between 1992 to 2015 at 300 m spatial resolution, has been found be more suitable for the work.

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 m3(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 m3/day for freshwater. This value is larger than the values given by the authors (6 307 m3/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.

Response: The high pace of urbanisation and deforestation at the catchment led to the choice of the RCP8.5 as the projection scenario. Notwithstanding, we are also including trends in RCPs 2.6 and 4.5 in the revised manuscript. Previously, the landuse input map was used for the projection of future hydro-climatic trends. However, different landuse scenarios are being considered in hand with the different RCPs to give consolidated conclusions to this study.

Specific and technical comments:

Figures 1 and 4 are too small and not readable.

Response: Changes have been made in the manuscript and these figures are enlarged.

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

Response: Duly noted and these would be shown in the revised manuscript.

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.

Response: This will be improved.

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

Response: This would be done in the revised manuscript.

Table 1: Please, specify the data resolution.

Response: The data resolution will be added in Table 2.

Table 6: Please, specify the units.

Response: The units will be added.

Page 2, Line 11: "management".

Response: The word "mangement" has been modified in the text to "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.

Response: The text has been updated.

Page 4: There is an error in the equation.

Response: The term "i+1" has been modified to the correct form "i=1"

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).

Response: The much simpler Hargreaves ET method is currently being employed and hence the wind variable would not be used anymore.

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

Response: The 5 years was chosen to ensure uniformity in the baseline and future projections timeseries (both of which have 31 years).

Page 8, Line 9: "Penman-Monteith"

Response: The name has been modified.

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.

Response: West African rainfall is precarious and has an inhomogeneous distribution. However, based on the size of the study, there was very little variations within the catchment area. Gridded data over Ghana is now available as reported in Aryee et al., (2017). This would be therefore used for the model run over the new catchment area (69 km^2).

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.

Response: Soil within the study area falls under "D" in the Hydrological Soil Group. This type has very slow infiltration rates when completely wetted. Hence runoff at the catchment is not of the Hortonian.

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

Response: This is well noted.

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

Response: According to Abbaspour (2015), the p-factor threshold for streamflow is a dimensionless quantity and can be expressed in decimal fraction or percentage (ie. 0.70 or 70%).

References: This section lacks many volume numbers, page numbers, editor or doi. Be careful. Throughout the manuscript, be sure to give reference**s.**

Response: The references is being updated accordingly.

REFERENCES:

Abbaspour, K.: SWAT-CUP, eawag, 2015**.**

Aryee, J.N.A., Amekudzi, L.K., Quansah, E., Klutse, N.A.B., Atiah, W.A. and Yorke, C., 2017. Development of high spatial resolution rainfall data for Ghana. *International Journal of Climatology*.