

# ***Interactive comment on “Hydro-Climatic Modelling of an Ungauged Basin in Kumasi, Ghana” by Marian Amoakowaah Osei et al.***

**Anonymous Referee #1**

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## **OVERVIEW**

The manuscript investigates the impact of climate changing on the water resources management of the Owabi catchment in Ghana. Specifically, the SWAT hydrological model was calibrated with past streamflow observations (from a neighbouring basin) and then used for simulating the different component of the hydrological cycle (precipitation, evapotranspiration, infiltration, ...) for past and future climate conditions (ob-

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tained by GCM for different scenarios).

## GENERAL COMMENTS

The manuscript investigates the impact of climate changing in an ungauged basin in Ghana. As the study area is in a developing country, with missing data, the obtained results are surely of general interest. However, the paper fails in showing new scientific results and the scientific soundness should be improved.

I listed below several major comments that should be addressed to make the paper results more robust and significant. Additionally, I would suggest converting the paper as Technical Note as it seems to me more appropriate.

1) The paper uses a single GCM. The uncertainty of this choice might be significantly large. What are the results for different GCMs? Do they go in the same direction? This must be tested.

2) The analysis is carried out for a very small basin (70 kmq). The spatial scale of GCM is much larger. It introduces further uncertainties that should be considered and discussed. For instance, the use of Regional Climate Models might be needed. As before, it must be investigated and assessed.

3) Discharge observations from a neighbouring basin are used for SWAT model calibration and validation. However, no details are given on the used dataset, and how it is used as a proxy of discharge observations for the investigated basin. It should be

clarified and clearly specified.

4) Overall, several details are missing in the explanation of the methodology. For instance, two different land use scenarios are considered but no information is given on the criteria used for the selection of such scenarios. Similarly, for the selection of a single GCM. A single hydrological model. A single downscaling approach (not explained properly). All these aspects must be clarified.

5) SWAT model is calibrated with discharge observations only (and not for the same basin). In the paper it is speculated what are the different contribution of precipitation and evapotranspiration on surface and subsurface runoff. By using only discharge data for model calibration, it is not possible to give information on the different components of the hydrological cycle. Different SWAT parameterizations might provide the same performances in terms of discharge simulation while providing very different shares in the hydrological components. The overall discussion should be removed and totally revised.

Several specific comments and corrections should be also addressed. However, I believe the paper in the current form should be significantly modified and, hence, I have not included the specific corrections at this stage.

I understand that the review is not positive. However, I believe there is a strong potential in the performed study as we surely need to investigate what will be the impact of climate changing in developing countries, and this study might provide an important contribution in this respect. Based on the above comments, I believe the paper needs a major revision before to be re-evaluated for its technical content.

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