Hydro-Climatic Modelling of an Engaged Basin in Kumasi, Ghana.

This manuscript presents an application of the SWAT hydrological model on the Owabi basin in Ghana. Once calibrated, the model was fed with climate change scenarios to assess the future availability of water on the basin. In addition, two hypothetic future land-use maps where used in combination with climate scenarios, to also assess the impact of a change in land-use on water resources. The first land use map corresponds to the current observed land-use map in the basin, thus assuming no evolution of the land use over time. The second land use map assumes extensive urbanisation of the basin in the future. The authors explain that as such, the Owabi basin is ungauged, the closest streamflow gauge being located 11 km away.

In my opinion, this manuscript is unfortunately not appropriate for publication in HESS and should be rejected. There are several reasons for that, the most important being that I find the level of originality and innovation of the work to be quite low. I would encourage the authors to submit a revised version of their manuscript to a smaller journal specialized in applications and/or case studies.

Major comments:

1. The originality of the work is low

In my opinion, this manuscript presents the application of a well known model to a new catchment. As such, there is nothing new with that. Commonly available data with very little pre-processing, as well as a standard calibration process were used. Readily available weather data for climate change scenarios were used, again with no pre-processing. There are already several papers in the literature focused on using hydrological models with climate scenarios to project future water availability.

I encourage the authors to follow the scientific method: what do you want to verify? State your hypothesis! What/where is your contribution? I advise mentioning explicitly in the introduction what your contribution to the advancement of science is.

2. Some methodological/conceptual elements need clarification

- The Owabi basin is ungauged. To me, this is a central issue for this work, but only very little information is provided in the manuscript about that. For one thing, although it is mentioned that the nearest gauging station is located 11 km away from the Owabi basin, on another basin called Offin. a map showing its location is not provided. Such a map would be very useful. In addition, information about the similarities and differences between those two basins (area, slopes, vegetation, soil, etc) should be provided. Finally, although you are using an existing method for transferring streamflow from Offin to Owabi, I believe you should nonetheless describe the method succinctly. Much more emphasis should be given throughout the manuscript to the fact that the basin is ungauged and that the calibration of SWAT was thus performed using streamflow (and corresponding meteorological observations, I would assume) from another basin.

- To me, although you mention bias correction for the climatic projections on page 7 line 10, it is not clear how the quality of those simulations was assessed. I would have liked to see much more details regarding those biases (with graphs and numbers) and also more details about the efficiency of the bias removal process. In addition, although you mention on page 7 that « These were projected under three Representative Concentration Pathways (…) »

you do not provide any detail about what those projected scenarios represent (for instance, which one is worst than the other in terms of greenhouse gaz emissions). I would have appreciated more details.

- You also mention on line 12, page 7, that distribution mapping assumes a gaussian distribution of the dataset. Have you verified that the datasets indeed follow gaussian distributions? Again, more details regarding that, alone with test results, should be provided. Why using two bias correction methods instead of one? Can you provide comparative graphs of the outcomes of those methods? How do they compare?

- I do not find Appendix A to be useful, as it contains basic equations that are very well-known in hydrology. I would advise removing it.

- The SWAT model operates on a daily time scale but you analyse data at a monthly time scale. I also understand that you might have access only to monthly streamflow observations for the Offin basin. I would have appreciated much more detail about the choice of the monthly timescale and how it impacts model calibration and data preparation, as well as post-processing of the outputs (simulated streamflows).
- The authors mention an uncertainty enveloppe on simulated streamflow. This envelope is not displayed on figures, and the methodology to compute it is also not explained. Again, more explanations are needed. The reader can only guess that it was obtained through the use of SWAT-CUP (page 8).

Specific comments

1. Typos/spelling mistakes: I would advise a very thorough linguistic revision. These are only a few examples:
   - The word « streamflow » is sometimes spelled « stream-flow ». Please check the entire manuscript and ensure consistency.
   - Page 1, line 15, page 8 line 15, page 10 line 3 and several other instances: please replace « at the catchment » by « at the catchment scale » or « on the catchment », depending on what you mean.
   - Page 2 line 28: on the Bani… (not at)
   - Page 2 line 30: « but sometimes highly overestimated… »
   - Page 3 line 2: « conclusion IS given in section 4. »
   - Page 3 line 5: Correct « … in Ghana. redIt comprises of the forest »
   - Page 8 line 15: remove comma after « that »
   - Page 8 line 16: correct « … soil group of type D… »
   - Page 9 line 15: correct « …the root nature of the forested trees …». Perhaps by « the forested area » or « trees in the forest ».
   - Page 10 line 21: « The catchment topography ranged from … » should be corrected to « The catchment topography ranges from … »
   - Page 14 line 3: Replace « welcoming » by « welcome »
   - Page 15 line 13: Replace « projection » by « projected »
   - Page 16 line 2-3: The sentence « Specifically… for the catchment » is confusing and needs rephrasing. As written now, I understand that the model can « simulate its own calibration ».

2. Page 1 line 2: « Major stakeholders… » please add a reference.
3. Page 3 line 8: There is a problem with one reference « Commission » is not a valid author name.

4. Page 4 after equation (1): « t » should be in italics. Also, there is a sequence in which you name 5 symbols but only mention 4 definitions (« Rday, Qsurf… »)
5. Page 6 line 6: « … it has been revealed that regionalization and other genetic networks… » I don't understand what you mean by genetic network. Please clarify.
6. Page 8 line 1: A reference is needed for the SUFI-2 algorithm.
7. Page 8 line 12: I suggest nuancing the sentence « Unlike rainfall that is easily measurable… ». There are lots of issues regarding accurate rainfall measurement (for instance under-captation and lack of spatial coverage of ground stations).
8. Page 10 line 15: You say that 14 parameters have been selected from literature. First, you need to cite some supporting references. In addition, again, this is much too vague. Why are those parameters so important compared to others?
9. Page 10 line 21: Given its importance, the long name of parameter CN2 should be mentioned (even if it is in the Appendix). In fact, this table should be in the text and not in an appendix. Incidentally the number of the Appendix as referred to in the text is not good (it is table 5, not Appendix 5).