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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of climate change on water resources in a tropical West African catchment using an ensemble of climate simulations" *by* Y. Yira et al.

Anonymous Referee #2

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The paper presents a climate impact assessment for two climate warming scenarios focusing in water availability in West Africa and in particular for a small scale West African catchment, where no studies are available about this topic so far. The authors have put a lot of work in dealing and examining state-of-the-art climate models and their uncertainties for the region and have carried out various analyses which are well presented in the figures. The analysis includes good approaches for the examination of specific hydro-climatic indicators (incl. water-energy budget) and their uncertainties as well as the application of a hydrological model. The paper is well written, in particular the introduction and discussion, and the methodology is well presented. The figures and tables are accurate and support the understanding of the results.

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However, the paper includes some inconsistencies, in particular concerning the interpretation of the results, and further shortcomings in scientific and structural aspects, which need to be revised. Therefore, the paper should be subjected to major revision.

My major request is that the authors should especially revise these major points:

1.) The first point is related to scaling issues which makes the paper difficult to understand when reading it for the first time. In the article actually includes two evaluations on different scales: one evaluation of available climate models on large scale (Climate data: 250x200km = 50.000 km2) and one hydrological analysis for the small Dano river catchment (200 km2). A box of 20 climate model grid cells were correctly used for the analysis to capture the spatial climate variability of the climate models, as stated in the Discussion (p. 12, L. 367) and examined for a larger region. In a second step, the small Danu catchment was examined using a hydrological model and climate model data of one grid cell as meteorological driver. These two analyses are partly mixed up in the presentation and interpretation of the results and need to be structured and distinguished in a better way.

2.) The authors carried out a bias correction for the climate models for the region.

Here it is described in the methods: p. 4, L.111: "Although the national observation network includes several rainfall gauges and synoptic stations, solely the data of the Dano station were used as it is located in the study area."

So, is it true that for bias correction of the climate model data (20 nodes of climate models -> 50,000km2) only the data from one meteorological station was used? If yes, the measured basis data for bias correction may be representative for the small Dano catchment, but lead to incorrect bias correction transfer functions if applied for the whole region.

3.) The hydrological model applied in the study has been calibrated using only three years of daily discharge data and validated for just one year in a previous study (Yira et al., 2016). In this study the model has than been used to project runoff changes under changing climate conditions until 2050. I suppose that this very short calibration and validation period was chosen due to missing measuring data in the catchment. Generally, in data scare regions a weighing is necessary between scientific accuracy and best scientific information available for the region under given conditions. Nevertheless, it may be questioned if the model's ability is adequately validated to project long-term runoff pattern in particular under changing climatic conditions in a monsoon region. Of course the major source of uncertainty in hydrological impact assessments is coming from the climate models, which are in West Africa related to very high uncertainties already. As for many African regions, the examination of available climate models in this study reveals very high uncertainties in the projections and no clear patters in the directions of the change. This may sound harsh but my point is that, as the input data as well a calibration and validation data for the hydrological model are quite low, the additional value of using the hydrological model to examine impacts on water resources instead of just analyzing the climatic water balance and water-energy budget out of climate model data for the region is not clearly highlighted in the paper. Furthermore, this point of additionally increasing uncertainties in the hydrological analysis is not discussed in the paper.

4.) The paper includes considerable inconsistencies in particular concerning the interpretation of the results which need to be revised.

In the conclusions the authors correctly conclude: p. 14, L. 438ff: "(..) the lack of agreement among the models with regard to the projected precipitation change signal creates a considerable uncertainty about how the catchment discharge will evolve by 2050s. As discharge in the catchment is strongly determined by precipitation, no clear trend in future development of water resources can be concluded due to the high variability of the different climate models and scenarios. Therefore, potential increase and decrease of future discharge have to be considered in climate change adaptation strategies in the region." And further in the Discussion: p. 10, L. 315: "(..) individual model errors of opposite sign cancel each other out (..). (..) the climate models ensemble means should not be considered as an expected outcome."

As correctly concluded, the results, in particular for RCM 8.5, are too uncertain to give any direction for future hydrological development and, hence, the mean values over the climate models cannot be used to give a direction of future changes. These statements are not consistently kept over the article, including the Abstract.

As example: Discussion: P. 13, L. 413ff: "The climate models ensemble means projects a precipitation increase about 1.5% under RCP8.5 with a resulting discharge decrease of 2%. This indicates that the catchment ecosystem is able to optimize the use of water and energy available in the environment (...)." Abstract: p.1, L. 24f: "The RCMs-GCMs ensemble average suggests a +7% increase in annual discharge under RCP4.5 and a -2% decrease under RCP8.5. (..) Therefore, potential increase and decrease of future discharge has to be considered in climate adaptation strategies in the catchment."

Hence, also the Abstract of the article should be revised according to the uncertainty of the results and the conclusions as given in the Conclusion section.

5.) In the Discussion the authors state: p.12, 362ff: " In the view of the general good simulation of historical discharge for the climate models ensemble, it is worth noting that running the hydrological model with simulated climate data of one node (..) has reasonably bridged the discrepancy between RCMs-GCMs data resolution and hydrological modeling domain. Therefore, the approach can be considered as eligible for climate change impact assessment for small scale catchments."

If I understood the methodology correctly, there were no hydrological measurement data available for the historical period, so the runoff, which was simulated with the input data from one weather station, was compared to the runoff simulated with one node of the bias-corrected historical runs of the GCMs-RCMs. As the bias correction for the climate models was carried out using this weather station data, the hydrological input data certainly turn out to be very similar as they have been adjusted by the bias correction (by definition!). Hence, this "proof" seems to me redundant and not convincing.

6.) Concerning the conclusions for the additional value of bias correction when analyzing the relative changes between the future and reference period which was one of the main objectives of the study: p. 3, L. 87: "[this study] has the following objectives: (. . .) (v) investigate the effect and necessity of bias correction on the detected signal."

In the Introduction the authors cite p. 2, L. 59 "unless the precipitation from climate models are bias corrected, results from hydrological simulations are unrealistic": This statement has been shown not to be necessary true if the relative changes of the climate signal are used as examined by e.g. Hagemann et al. 2011 and Muerth et al. 2013, which are cited in p.5, L. 163. As one main objective of the study, the pro and cons of bias correction should be discussed more extensively in the introduction and/or discussion.

To evaluate the impact of bias correction, the hydrological model was additionally run with not bias corrected future climate of one single climate model which was randomly selected among the 6 RCMs-GCMs (p. 5, L. 163). As this analysis is mentioned as one main objective of this study, I suggest carrying out this analysis for the other 5 climate models to get more reliable results on this topic. Otherwise the conclusions should at least be toned down (e.g. "This result supports further studies, which have shown . . .") and this topic should not be headed as main objective of this study. [Conclusions p.14, L. 432: "This result indicates that it is safe to perform bias correction; it also points out the "non-necessity" of performing bias correction in order to detect future discharge change signal in the catchment."]

Minor comments:

p. 3, L. 84 - please correct "echohydrologocal"

p. 4, L. 123f: "(..) data resolution (0.44°, about 50 * 50 km2) and the hydrological

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modeling domain (about 18 * 11 km2)" Please correct "km2" to "km".

p. 9, L. 268: "The intra-annual change in discharge appears strongly determined by the precipitation change signal (Fig. 8)." Fig. 8 shows monthly discharge changes but not the reason for these changes. Reasons for these changes in discharge could also be e.g. higher evaporative demand due to higher temperatures.

p. 10, L. 289: please correct "Eco-hydrologic"

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