Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-591-RC1, 2017

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "Scenario-based impacts of land use and climate changes on the hydrology of a lowland rainforest catchment in Ghana, West Africa" *by* Michael S. Aduah et al.

Anonymous Referee #1

Received and published: 1 December 2017

The manuscript "Scenario-based impacts of land use and climate changes on the hydrology of a lowland rainforest catchment in Ghana, West Africa" by Aduah et al. deals about the separate and combined analysis of impacts due to climate and luse use change using the ACRU model. For this, a calibrated model for the Bonsa catchment was used (Aduah et al., 2017, companion paper).

In general, impact studies for African catchments are of broad interest since adaptations plans regarding water management for the future will be necessary in the context of climate change. Hydrological or ecohydrological models can be a useful tool to support management decisions. However, the usefullness of model results strongly

Printer-friendly version

Discussion paper

depends on a reasonable application of the models and a thorough analysis of the model results. In this regard I have two main concerns that need to be addressed or clarified by the authors:

1. All presented results are based on modelled monthly streamflow. Model calibration was presented in a companion paper. It was reported that "validation based on the daily time step did not generate satisfactory performance as NSE of 0.14 and 0.31 were obtained during calibration and validation, respectively". As a consequence monthly time steps were used since performance was better.

In my opinion, it is not good modelling practice to leave out poor model performance on a daily time step and to present satisfying model performance on a monthly basis. There must be a reason for poor model performance and in this regard, the authors need to clarify, if this poor performance may have implications for all following applications and conclusions. How can the authors be sure that hydrological processes are adequately simulated? Obviously there are not well simulated. Otherwise, model performance for daily time steps were much better.

Additionally it is not good modelling practice to use only a small number of performance measures, especially if all selected measures are focused on peak and high flow. I respect the circumstance that the investigated catchment may be not intensively monitored and that data scarcity may be a problem. However, there are additional ways to make sure that the model behaves reasonable and of course realistically (e.g. using constraints, rules-of-thumb, multi-site calibration, model output such as discharge components or hydrological components over time).

2. Due to coarse temporal resolution, all derived conclusions do have a more general character such as "wetter" or "longer dry periods". I wonder if this is really a good basis to develop management plans for a catchment. Other short-time effects such as flood events or extreme precipitation events cannot be considered at a monthly scale. Consequently, it is impossible to discuss implications of those events on agriculture

Printer-friendly version

Discussion paper

or humans even if they might be more relevant than the general tendencies that are presented. Leaving out daily resolution might be also the reason for more or less similar tendencies for all climate scenarios. Based on the previous points, I do not share the opinion that this study provides a platform for further studies since high uncertainties are given due to methodical limitations (monthly resolution, exclusion of additional model output for further analyses).

In the following there are some minor comments:

L.34: I wonder if there are no recent studies that underline these statements. Examples are:

Gloria Salmoral, Bárbara A. Willaarts, Alberto Garrido, Björn Guse, Fostering integrated land and water management approaches: Evaluating the water footprint of a Mediterranean basin under different agricultural land use scenarios, In Land Use Policy, Volume 61, 2017, Pages 24-39, ISSN 0264-8377, https://doi.org/10.1016/j.landusepol.2016.09.027.

Pablo A. Mendoza, Naoki Mizukami, Kyoko Ikeda, Martyn P. Clark, Ethan D. Gutmann, Jeffrey R. Arnold, Levi D. Brekke, Balaji Rajagopalan, Effects of different regional climate model resolution and forcing scales on projected hydrologic changes, In Journal of Hydrology, Volume 541, Part B, 2016, Pages 1003-1019, ISSN 0022-1694, https://doi.org/10.1016/j.jhydrol.2016.08.010. Hartwich, J., Schmidt, M., Bölscher, J. et al. Environ Earth Sci (2016) 75: 1071. https://doi.org/10.1007/s12665-016-5870-4 I am pretty sure that there are many other studies that may be cited here.

L.49: I would use K as unit.

L.174: It seems very vague to me, if "Temporal Dynamics" is the right term for monthly and annual discharge.

L.180: Does this finding indicate that a daily resolution is appropriate to reduce model uncertainty?

L.186: It would be interesting to see if there are additional negative impacts on agriculture (e.g. higher floods, extreme precipitation) that limit the agricultural productivity. Since this analysis is based on monthly resolution, this aspect cannot be considered and consequently, the results are limited to more general statements (e.g. length of wet season).

L.259-269: This part is not a discussion but a summary of the results.

L.287: Of course it is of advantage to consider additional aspects beyond the outlet streamflow to discuss climate change and land use change impacts. However, the authors left out other approaches such as having a look at model output (streamflow components, water balance).

L.294: I do not share the opinion that this study provides a platform for further studies since high uncertainties are given due to methodical limitations (monthly resolution, exclusion of additional model output for further analyses).

L.329: In this regard I am not with the authors since model performance was evaluated with only a small number of performance measures and only for monthly resolution at a single gauge. Consequently, I do not see a satisfactory calibration.

Comments on figures and tables:

Fig.4: very unsharp, needs to be integrated in a higher Resolution

Fig.5: Why do the authors scaled up to 3000 m^3/month? 1500 would be enough and would allow having a closer look at the discharge. Please be consistent with scale resolution (e.g. fig a,b). It should be mentioned that it is modelled discharge.

Fig.6: I do not understand the term "1 in 10 year high".

Fig.9: Information in the right upper part is already given in the caption

I hope that the authors understand the listed comments as a recommendation to rework and improve some fundamental points of their manuscript. Hopefully, this study will be

published after a major revision. I wish much success in this.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-591, 2017.

Printer-friendly version

Discussion paper