Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-152-RC2, 2016
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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Simulated Hydrologic Response to Projected Changes in Precipitation and Temperature in the Congo River Basin" *by* N. Aloysius and J. Saiers

Anonymous Referee #2

Received and published: 22 May 2016

General comments

The authors address future change in water availability in the Congo Basin. This topic is welcomed given the relative lack of research for this important region. The authors have embarked on a thorough analysis using projections from 50 climate model experiments which are bias corrected and downscaled and run through a hydrologic model. As with any impacts study of this nature, there are a host of uncertainties and methodological choices which can influence the outcomes, and it is challenging to distill information about future impacts in this context. There are also different views amongst scientists as to the best way to approach these uncertainties. However, personally I feel that the balance of emphasis on uncertainties is not quite right in this study, and would like to see more discussion/emphasis on the climate model uncertainty (and less emphasis on the multi-model mean), as well as more analysis of observational uncertainty. Therefore I suggest major revisions. Please note that my background is in climate science so I will mainly comment on this component of the study, and do not have the relevant expertise to comment on the hydrological modelling.

1. Model uncertainty: Given the uncertainties associated with future climate, I think that the comments on the implications of the findings, particularly in the abstract and conclusions, are too strong. The "challenges" described for planners in the abstract occur only if the projections are valid (which we won't know for 50 years). The authors also make several comments about the importance of providing "details" for planners. I disagree. I think it is more important that planners are aware of uncertainties in future climate, and would benefit more from information about the range of future projections than the multi-model mean. (This is in line with a body of researchers and literature discussing such issues e.g. Weaver et al. 2011; Dessai et al. 2009; Knutti et al. 2008).

The authors have quite a large ensemble of projections from their modelling which could be made much more useful in this regard. I think it would be more useful if they commented on the size of the uncertainty and what this means for planning – are there any regions for which there is not a great deal of model uncertainty, where planners can prepare for wetter or drier conditions? Or is there also uncertainty in the direction of change which might mean that adaptive/robust planning strategies are more appropriate? How does the uncertainty from climate models compare to other uncertainties e.g. if a different hydrological model were used?

In general I think they should put more emphasis on understanding the range (e.g. Figure 7 which is useful) and less on the multi model mean (e.g. Figure 6, which should include a measure of uncertainty).

We have revised the text and abstract to highlight the projection ranges and the uncertainties planners will encounter. Figure 6 has been modified to show model projection agreement in runoff change. This figure also highlights the spatial variability in the direction of change.

The historical simulation period is from 1950-2008. The model was calibrated during the early part of the simulation period in order to take advantage of available observed river flow data at 30 gage locations within the basin. The model simulations were validated outside the calibration period at 30 gage locations (Figure 1 and 2). The region had sufficiently detailed data during early part of the

simulation period [*Alsdorf et al.*, 2016; *L'vovich*, 1979]. Satellite measurements, sparse ground-based measurements and reanalysis products provide the most reliable climate data for the reminder of the simulation period [*Alsdorf et al.*, 2016; *Munzimi et al.*, 2014].

We only used one hydrological model. However, recent research suggest that projection uncertainties dominate compared to other sources of uncertainties (e.g. model structure and parameters) in hydrologic projections [*Maurer and Pierce*, 2014]. Suggested references have also been used to improve the discussion section.

2. Observational uncertainty. The first sentence of the paper states that efforts to understand the impacts of climate change in the Congo Basin are hindered by data availability. However, the authors do not make clear in the paper how they have overcome this, or the extent to which their findings are valid given observational uncertainty. They use an observational dataset from Sheffield et al. and (I think) use this for (a) bias correction (b) temporal downscaling to daily data and (c) sub-selecting climate models based on their ability to represent the region. Therefore, the observations might have a very important influence on their findings.

It is generally accepted (e.g. Washington et al. 2013) that availability of observed climate data in this region is a huge problem which might prevent subselection of models or bias correction. How can we say which model is more valid when there are basic questions remaining about the quantity of precipitation or where the precipitation maximum occurs? What dataset should we use assess and correct biases when there are large differences between the observational datasets used? The Sheffield et al. dataset does sound like an impressive undertaking and an important initiative but in the absence of rain gauge records it is difficult to validate it for this region, so it it still just one estimate of the observed state. I think the authors should, as a minimum, comment on the extent to which this dataset is reliable for the region and the extent to which their results might be influenced by observational uncertainties. They could also repeat their correction analyses with an alternative observational estimate and see whether this influences their results.

I am particularly concerned about the temporal downscaling to daily data, and think the authors should comment on the extent to which this is reliable, give that our understanding of day-to-day variability in precipitation/organisation of convection/meso-scale convective systems in this region is just beginning.

As mentioned in the earlier response, the region had sufficiently detailed ground-based observational data (e.g. precipitation and river flows) during early part of the simulation period. Satellite-based and limited ground-based observations are used to develop historical precipitation data used in our study. The dataset is developed and evaluated using multiple observation-based and reanalysis products (TRMM, GPCP, CRU, NCEP-NCAR and the second Global Soil Wetness Project) [*Sheffield et al.*, 2006]. During the development of the this dataset, the NCEP-NCAR precipitation product was examined and corrected for total monthly precipitation and monthly rain day statistics using CRU, GPCP and a 15-year gage-based dataset. The downscaling process also took into consideration the spatial consistency.

The lack of observational data (both precipitation and river flow) during the late 1970s and 1980s is a constrain and a limitation in this region. We have discussed these limitations and constraints in the manuscript.

Specific comments

p. 4. Line 50. “require detailed information” – perhaps rephrase. If the information is not credible then details could be counterproductive. So I think better to say “would benefit from detailed information”

Revised

p. 4. Line 54. “predictive” and “forecast” – suggest change to “project” since we cannot forecast or predict on these timescales, only “project” what if under certain emissions scenarios. Suggest changing throughout.

Revised

p. 9. Line 162 – I am not sure what is meant by “medium mitigation” for RCP4.5.

The phrase is revised as “mid-range mitigation emission”. The paragraph is revised to make clear the two emission scenarios.

p. 11. Line 190 – does this refer to bias corrected precip? If so I think this should be highlighted. Does it mean much if bias corrected precip fits with observations? Since it has been corrected using these observations?

The GCM-simulated annual precipitation refers to the bias-corrected values. The paragraph has been revised to make this point clear. We used a statistical bias-correction method to correct monthly GCM-outputs [*Li et al.*, 2010]. The procedure is described in the methods section and in the SI.

p. 11. Line 193. “The modeled inter-annual variability among the climate models (vertical bars in Figure 2) lies within the range of the observed variability” – Looking at the figure I am not sure this is strictly true. I can see a few examples where the error bar for the models is larger than for the observed.

In Figure 2, we show, among the 25 GCM outputs, the largest (red vertical bars) and smallest (blue vertical bars) values. As noted, there are some GCM outputs that show larger variabilities.

Figure 2 – please clarify the meaning of the modelled error bar. The caption states that it is based on the minimum and maximum range of interannual variability from the models. Is there anything to show the range of mean/climatological values for the models? And how does this compare? (similar comment for Figure 3b)

The vertical bars show mean ± one standard deviation of GCM-simulated annual precipitation during the historical period (1950-2005). The red bars denote the largest variability (highest value of std. dev.) within the 25 GCM outputs, and the blue bars denote the smallest. The horizontal bars shows the mean ± one standard deviation for the observed precipitation during the same historical period. Each black point indicates the mean annual precipitation within the drainage areas at gage locations showed in Figure 1. The text and figure captions have been revised.

p. 13. Line 239 – why is the IQR used? What is the full range?

We chose to present the inter quartile range to highlight where the bulk of the projection values lie. The full range of precipitation projections varies between a 3% decrease to a 6% increase in the near-term (2016-2035). The mid-term (2046-2065) changes are -5% to 7.6% for RCP4.5 and -6% to 9% for RCP8.5, respectively.

Figure 7 – nice figure. Is there a way to make historical plot clearer?

Figure has been revised.

p. 16 Line 293 – I am not sure that MMEs reduce uncertainty. It’s more that they help explore and reveal uncertainty.

We have revised as per the reviewers suggestions.

p. 16 Line 304 – I think it is overstating it to say that these models reliably simulate regional climate. We don’t have good enough observations of the regional climate to judge this. And, in any region, subselecting models is usually about taking the ones which most reliably simulate regional climate, rather than being confident that they are good enough.

We evaluated the annual, seasonal and monthly simulations of precipitation and temperature by the 25 GCM in the Central African region in a separate manuscript [*Aloysius et al.*, 2016]. Previous works in the Central Africa region highlight that model skill in simulating precipitation are partly dependent on how they replicate teleconnections with sea-surface temperature (SST) departures, particularly in the North Atlantic and Indian Ocean sectors (e.g. *Balas et al.* [2007]; *Dezfuli and Nicholson* [2013]; *Hirst and Hastenrath* [1983]; *Suzuki* [2011]). Our companion manuscript [*Aloysius et al.*, 2016] explored the linkages between precipitation and SST departures, and identified a subset of GCMs that simulate precipitation well.

We revised the discussion taking into consideration i) the above points and ii) the reviewer’s comments.

p. 17 Line 326. This is quite an odd paragraph which starts of talking about implications of findings (from MM and SM?) and then finishes by saying we can

reduce the range of projections from MMEs. Perhaps this should be reconsidered to suggest more nuanced conclusions about the implications of the findings which incorporate un- certainty? It is also not clear, when the author is discussing the potential to constrain model ensembles using knowledge of mechanisms that moderate the regional climate system, whether this is something they feel they have already done, or something that needs to be done. If the former, I'd suggest that their subselection procedure warrants further attention in the paper.

This section has been revised.

Figure 8 – quite a lot of information here. Could it be distilled to extract the main message?

Figure is revised.

p. 19 Line 363. "with sufficient details". I disagree. Providing details to planners may be misleading if there is too much uncertainty to give details. Better to help planners understand the uncertainty?

This section has been revised to highlight projection uncertainties between GCMs and emission scenarios.

p. 20 Line 377. "The analyses presented in our work increase the degree of confidence in using the results for policy and management." This is unsubstantiated.

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