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Discussions

Interactive comment on "Empirical streamflow simulation for water resource management in data-scarce seasonal watersheds" *by* J. E. Shortridge et al.

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We thank the reviewer for their prompt and thoughtful review of the manuscript. The author raises a number of issues that could be addressed through clearer presentation on our part, so we would like to take this opportunity to respond to the specific points raised.

1."The good performance of the climatological model is almost completely a result of the low interannual variability in the flow regime as evidenced by Figure 3, but this issue is never mentioned."

Response: The reviewer makes a valid point that Figure 3 makes it appear as though there is very little interannual variability in the region's hydrology. However, this actually is not the case, particularly when one considers the other rivers assessed as part of the study, each of which has approximately three times as much interannual variability (based on the coefficient of variation for annual flow volumes) as the Gilgel Abbay (see table attached as Figure 1). The manuscript's Figure 3 was just presented as an example to demonstrate the bias in standard formulation model predictions of wet season flows, and it could just as easily be replaced by a different river/time period to demonstrate that this phenomena exists (see Figure 2 attached to comment). We thank the reviewer for bringing this issue to our attention, and will replace the hydrograph used in the manuscript's Figure 3 with the figure below in an effort to avoid the impression that interannual variability is very low. We also plan to add information on interannual variability in flow for each river to Table 1 of the manuscript.

2."Some of the other comments in the paper about how the empirical models can be used to assess physical realism are also, in my opinion, rather tenuous. 'Runoff increasing with higher precipitation levels and decreasing with higher temperatures' (page 19) is hardly a measure of physical realism... I therefore cannot agree with the authors that their models can be used to characterize 'watershed behaviour in a manner that could shed light on underlying physical processes' (page 18). If that is the case, what are the processes? Are the dry season processes groundwater driven or drainage from wetlands?"

Response: We acknowledge that the models in their current formulation cannot be used to assess complex questions about physical hydrological process in the basins studied. However, we disagree that the simple relationships identified in the models are not measures of physical realism, and point towards similar evaluations that have been used to assess empirical model performance in the literature. For example, Han et al. (2007) explore how ANN flood forecasting models responds to a double-unit input of rain, finding that some formulations respond in a hydrologically meaningful way to increased rainfall intensity, while others do not. Similarly, Galelli and Castelletti (2013) describe how input variable importance can be used to highlight differences in hydrologic processes between an urbanized and forested watershed. In both cases, the relationships identified were fairly simplistic (e.g., higher runoff with greater rainfall intensity; shorter time of concentration in urban versus forested watersheds), but are still important steps in characterizing the mechanisms by which models make predictions so that they are not "black-boxes" and confirming that these mechanisms make physical sense. Some of the questions the reviewer brings up, such as dry season streamflow contribution, could be evaluated through revised model formulation (eg., developing separate models for streamflow prediction in the wet and dry seasons), and this could be an interesting area for future research.

3."The argument that these types of models are good for places where there are good climate data but poor physical data may be valid, but the real question is how often do such situations occur and if you have good flow data, why do you need a model to make water resources decisions."

Response: While it is impossible to say exactly how often this situation occurs without a comprehensive review, this work was motivated by the specific data available in the Lake Tana region, where long records of flow and low-resolution climate data were available but detailed, ground-truthed spatial data on land cover and soil were not. In our experience this is not a rare situation: due to a combination of historical data centers (e.g., World Meteorological Organization reporting for climate and the Global Runoff Data Centre for streamflow) and more recent efforts to merge satellite data with in situ observations to monitor climate and hydrology (e.g., the Global Precipitation Climatology Project and the Global Land Data Assimilation System) one can often find acceptable climate data, even in data poor regions. Obtaining measurement-based estimates of soil hydraulic parameters or details on hydrologically-relevant land management activities can be more difficult. In this instance, there are two contexts in which historical flow data would be insufficient for decision making. In the short-term,

models are needed to take advantage of seasonal climate forecasts to more efficiently manage hydropower and irrigation schemes. In the long-term, changes in land-cover and climate mean that historic data are unlikely to be representative of future flow conditions. Thus, any estimates of how proposed long-lived infrastructure will perform in coming decades requires models to translate climate and land cover conditions into flow.

4. "I am not sure about the value of the climate change scenarios as they appear to me to be very simplistic and add very little to the study."

Response: We would like to clarify that these aren't climate change scenarios (which would describe plausible climate conditions expected to occur in the future), but instead measurements of the sensitivity and uncertainty of model predictions when forced with increasingly extreme climate data. Since one of the key motivations for using rainfall-runoff models is to understand how climate change may impact water resources, it is important to understand how model formulation contributes to this sensitivity and uncertainty. The analysis was thus kept intentionally simple, in an effort to avoid obscuring differences between models and implying that this analysis represented a projection of expected climate change impacts. While these issues could certainly be explored using actual downscaled climate model projections to make the assessment more representative of possible future impacts, we don't think that this additional complexity would add much to the inter-model comparison. We will make sure to make these points clearer when revising the manuscript.

5."On page 6 the authors suggest that empirical models can provide more comprehensive uncertainty analysis results. Why when there are many recent examples of rainfall runoff models being used for uncertainty analysis and the assessment of model results from a behavioural and non-behavioural standpoint."

Response: We acknowledge the reviewer's point that there have been a number of instances where uncertainty assessment has been conducted using physical rainfall-runoff models, but the statement on page 6 was referring specifically to the Lake Tana basin where there has been relatively little assessment of uncertainty in hydrologic modeling studies. This point will be clarified in the revised manuscript.

6."There are many places in the text where the word 'data' is treated as singular, while it should always be treated a plural (i.e. 'these data', 'date were', 'data area', etc.)." Response: We thank the reviewer for identifying this error; it will be corrected in the revised version of the manuscript.

7."The reference to the estimates of rainfall intensity on page 7 should be removed as this method will never give a proper estimate of intensity."

Response: We agree with the reviewer that the method is a very rough approximation of actual rainfall intensity. However, when presenting this work we have received multiple questions about whether rainfall intensity was considered in the evaluation, and thus think that this should remain in the manuscript to demonstrate that we did consider intensity to the degree that the available data allowed and found that it was not a useful addition to the models.

8."Page 8 refers to a log transformation of monthly streamflow to get a better match to normal, however, the distribution properties of the monthly flow data are not assessed."

Response: This information can be added to the revised manuscript as an appendix or supplemental material.

9."If NSE is considered such a bad statistic, why not use something else. Even NSE based on log transformed values can remove some of the bias to high wet season flows."

Response: We used NSE based on raw flow values (rather than log-transformed) because that is what has been used in other modeling studies conducted in the basin and thus seemed like the most appropriate metric for comparison. It should be noted that we don't necessarily disagree with the use of NSE as a metric, but rather the assumption that an NSE score greater than 0.5 indicates good model performance. This point will be clarified in the revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11083, 2015.

River	Mean annual flow (MCM)	SD of mean flow	COV of mean flow
Gilgel Abbay	1883	217	0.12
Gumara	236	71	0.30
Koga	114	31	0.27
Megech	172	54	0.31
Ribb	210	76	0.36

Figure 1: Characteristics of interannual flow variability in the five rivers assessed.

Fig. 1.

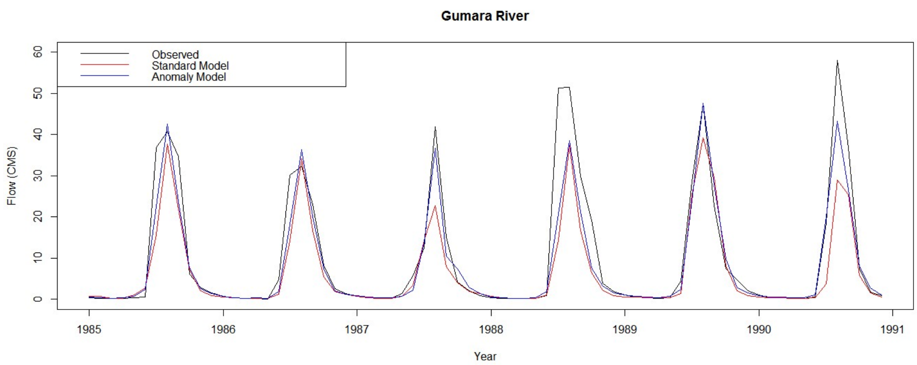

Fig. 2.