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Hydrology and Earth System Sciences Discussions

Interactive comment on "Projected impacts of climate change on groundwater and stormflow in a humid, tropical catchment in the Ugandan Upper Nile Basin" *by* D. G. Kingston and R. G. Taylor

Anonymous Referee #2

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The study presented by Kingston and Taylor addresses a highly relevant topic: Uncertainties associated with assessing the likely impacts of climate change on water resources in drought-prone regions. In general, the paper is clear and to the point. However, there are some severe problems with the modelling approach followed in this study.

The model which was run at daily time steps whereas only monthly input data were used. As a consequence, a down-scaling procedure had to be applied which is generally fraught with problems, irrespective of the chosen down-scaling approach. It is not clear why the available daily data were not used for model calibration. In fact, the

Nash-Sutcliffe efficiency of the runoff model was close to zero. It seems that a simpler model run at monthly time steps would have been more appropriate. This topic needs to be discussed in much more detail in the paper. The authors argue that the model nicely depicts the annual mean seasonal runoff (Fig. 2). That needs to be quantified. Again, it can be argued that a different model structure would have been more appropriate. E.g., low-pass filtering of the seasonal precipitation minus evapotranspiration pattern might yield equally good results but with much lower uncertainty.

The most crucial point of the GCM is related to precipitation which on the other hand is of outermost importance for any hydrological model. This has been discussed rather extensively in the literature and needs to be given more credit.

The title is too general and does not reflect the most interesting aspect of that study, i.e., investigating different sources of uncertainty for climate change impact assessment for that region (cf. last phrase of the abstract). Besides, using the term "groundwater" in the title is misleading. As far as I got it, there was no way to test the groundwater contribution to the stream other than by comparing with the hydrograph. Thus, that model output should be handled with outermost care.

For reasons given above, I recommend not to investigate the groundwater contribution and to skip figure 5. Figure 3 should be replaced either by a scatter plot or by giving the Pearson correlation coefficient between the two variables.

The quality of references is appropriate. But see comment above concerning precipitation uncertainty of GCMs .

Technical corrections: - P. 1918, l. 14; p. 1927, l. 21-26: Please give references for the Hargreaves, Penman-Monteith and Priestley-Taylor approach. What parameter values were used, e.g., for resistance in the Penman-Monteith approach? - P. 1918, l. 16: In case the Todd et al. (2010) paper is not accepted, more details need to be given here about the pattern-scaling technique.