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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

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Reply to comments on "Projected impacts of climate change on groundwater and stormflow in a humid, tropical catchment in the Ugandan Upper Nile Basin" by Kingston and Taylor

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Response to anonymous Reviewer #2

We thank the anonymous reviewer (#2) for their helpful comments and provide a point-by-point response to their queries below.

1. 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.

> The available daily data for the Mitano comprise a short period (1965-1980) of precipitation data within the catchment, and a longer series of temperature data that is located approximately 50km east of the catchment (at Mbarara). As noted in the response to Anonymous Reviewer #1, the precipitation time series is too short to enable a satisfactory model calibration and validation to be conducted. Use of temperature data remote from the catchment is not considered ideal either. As is explained in the revised MS in more detail (p6, lines 8-16), use of the CRU data set allowed a sufficiently long calibration/validation period, together with consistency with other studies in the wider research project that this study is part of.

2. The authors argue that the model nicely depicts the annual mean seasonal runoff (Fig. 2). That needs to be quantified.

> The close agreement between 30-year mean monthly discharge is clearly presented in Figure 2a, with the model and observed flow duration curves shown in Figure 2b. RMSE has been calculated for both monthly and 30-yr mean monthly flow to provide further quantification of what is shown in these two graphs (revised MS, p9, line 19).

3. 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.

> A simpler model, or simple P-E, would bring certain advantages in terms of reduced

uncertainty if precipitation and evapotranspiration data were available for the Mitano catchment. Given the hypothesised key role CRU TS 3.0 precipitation data as a cause of uncertainty in the Mitano hydrological model, however, it is unlikely that calculation of P-E would lead to a reduction of uncertainty in changes to freshwater resources in the Mitano basin. Furthermore, such an approach would also bring disadvantages in terms of reduced fidelity of the results (e.g. separation of fast vs. slow flow). The longest environmental data series in the Mitano catchment is the discharge time series, and so it can be argued that the most direct means of simulating Mitano basin freshwater resources is by modelling river discharge directly.

4. 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.

> We are not quite sure of the meaning of "outermost importance", but interpret this comment to refer to the primary importance of precipitation in driving hydrological variation. The original manuscript repeatedly noted the key role that precipitation has for driving the hydrology of the Mitano basin, both with respect to model calibration (page 1920, last paragraph; Figure 3) and scenario projections (page 1923, last paragraph; page 1925, lines 4-13; Figures 6 and 9). Additional references to the literature are made in the revised manuscript regarding the large differences between GCM precipitation projections (p15, lines 29 and 31).

5. 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.

> Title modified accordingly.

> Hydrograph-based classification of groundwater influence on streamflow is a standard and well-used technique (e.g. Chapman, 1999; Dewandel et al., 2003; Szilagyi et al., 2003; Sujono et al., 2004), and although not perfect, can provide a reliable indication of groundwater discharges (i.e. contributions to river discharge). Furthermore (and as noted in the original MS: page 1920, lines 13-19), results from baseflow separation of modelled discharge reveal a good fit to baseflow separation of observed river flow, and also compare favourably to a previously published model of the Mitano. Combined with the good fit of observed and modelled flow duration curves (especially at low flow levels – Figure 2b), we consider the inclusion of groundwater to be valid, and indeed to be a valuable contribution to the overall manuscript.

6. For reasons given above, I recommend not to investigate the groundwater contribution and to skip figure 5.

> See reply to point 5.

7. Figure 3 should be replaced either by a scatter plot or by giving the Pearson correlation coefficient between the two variables.

> The correlation coefficient (0.47) is given in the original text on p1921 (line 4). A plot of the time-series of the precipitation and discharge differences is preferred to a scatterplot because it demonstrates the near-synchronicity of precipitation and discharge differences (information that would be lost in a scatterplot). This is particularly important given that precipitation and discharge differences may not be exactly coincident in time because of the time lag between precipitation delivery and discharge at the Mitano gauge, and variation in this due to catchment surface conditions (e.g. existing soil moisture deficit/surplus).

8. 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?

> References indicating how the different PET calculations were made are included in the revised MS (p6, line 23; p16, lines 32-33).

9. 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.

> Additional references and further description are included in the revised MS (p6, line 27-32; p7 lines 1-3).

References

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Szilagyi, J., Harvey, F.E. and Ayers, J.F. (2003) Regional estimation of base recharge to ground water using water balance and base flow index. Ground Water, Vol. 41(4), 504-513.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1913, 2010.