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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modelling the Mara River Basin with data uncertainty using water levels for calibration" *by* Petra Hulsman et al.

Anonymous Referee #2

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This paper applies FLEX-Topo to the Mara River Basin and calibrates the model using stage data. Developing methodologies to deal with discharge uncertainties, particularly in poorly gauged catchments, is an important research area and there is scope for the results presented to be of interest to the research community. However, at present, I struggle to understand what the key research contributions are from the paper and some of the methodology is difficult to follow. These points are expanded upon below.

Main Comments

1. The main aims and goals of the paper are poorly stated. Developing a hydrological model for a particular region as stated as the main goal of the paper is not a 'Cutting edge case study'. Similarly, the key research contributions from the paper are not clearly highlighted within the conclusions. The authors need to think about the novel

aspects of the paper and two-three key messages they want the reader to take away.

2. The title of the paper is currently misleading – I would remove 'with data uncertainty' as you do not consider uncertainties in stage data and the analysis of precipitation uncertainties is limited.

3. There needs to be a broader introduction to data uncertainty in the introduction including rainfall uncertainty as this is considered later in the paper. Furthermore, there should also be a larger section devoted to model calibration and model diagnostics and particularly how to perform robust model evaluation in the face of data uncertainties.

4. A separate section on data would be useful. At the moment, different datasets are introduced at lots of different points throughout section 2 and section 3.

5. One of the reasons for calibrating the model to water level is to 'avoid' uncertainties in water discharge. However, by then calibrating the 'c' parameter for the Strickler formula surely you just replace one source of uncertainty with another. As stated in the paper, it is likely that this parameter is also compensating for large sources of uncertainty in your precipitation data so I wonder how robust the results are given all these different sources of uncertainty. This needs to be better discussed in the limitations.

6. Section 3.3 is really difficult to follow and certain model choices need to be better justified –

a. Why was NSE chosen for model evaluation? How appropriate is NSE for calibrating water levels?

b. Strickler formula on line 205 needs to be presented as a separate equation – what do 'k' and 'i' denote?

7. Results

a. Section 4.1. The authors state at a couple of points that 'the observed and modeled water depth were quite similar to each other'. How similar is similar!? It would be better

here to state NSE values as a quantitative measure of how similar they are.

b. Section 4.2. How many point discharge measurements were taken? While these can be useful in model calibration and evaluation – I don't think comparing a single point measurement to a whole month of modeled discharge was useful and the fact that the modeled results were 'within an order of magnitude of the point measurement' not a particularly persuasive argument that the model was performing well. I think these could be incorporated much better into the model evaluation framework.

8. I was surprised that given the amount of effort that went into defining HRUs and different model structures for the basin based on field observations and interviews, no results or analysis was presented on these different model structures. Was it just data uncertainty that lead to poor model performance or also the definition of model processes? How were model simulations improved by using two different model structures rather than one?

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