

Response to the comment of Referee #3

We would like to thank the reviewer for her/his positive and insightful comments on the manuscript. Below is our response to the issues raised in the review. The original comment is printed in plain font, our response is printed in italics.

This is a paper that is worthy of publication in HESS. The authors do an excellent job synthesizing existing literature on modeling low streamflow hydrology, and provide an interesting approach to assessing the impact of climate change on low streamflow prediction. Low streamflow prediction is inherently a challenging problem, and combining and assessing multiple approaches to forecasting low flows given potential climate change helps develop more holistic approach to low streamflow prediction. As such, I strongly recommend this paper be published in HESS, as it provides information useful to a wide variety of readers. Regardless, I do have a number of comments and suggestions that the authors might consider when revising this manuscript.

1) The three-pillar approach presented in this paper is not necessarily restricted to low streamflow estimation (i.e. it could just as easily be applied to flood flows or other hydrologic statistics). This should be made clear to the reader.

We agree that the overall approach is useful for a wider range of applications. We are now making this clearer in the discussion section of the paper.

2) One reason that low streamflow estimation is challenging is that they are typically driven by groundwater discharge processes (both recharge and discharge). These processes are difficult to understand and model due to their heterogeneous nature, and often these processes are overly simplified in rainfall runoff models (whose focus is typically flood or average streamflow prediction). Some discussion of this is warranted, as well as how these processes and their drivers are impacted by changes in climate.

We fully agree, groundwater processes controlling streamflow are often of a local nature modulated by the local hydrogeology, and the runoff model used is indeed a very simple representation of these processes. We are now acknowledging this in the discussion section of the paper and discuss potential effects of the simplification.

3) [NOTE: The following comment was written prior to this reviewer reading the entire manuscript. I am aware that this is discussed at the end of the paper (page 13096 line 13), but perhaps it should be discussed earlier since I continued to question this assumption throughout the paper.] An assumption of a linear trend in Q95 is made (equation (1)). Some discussion of the merit of this assumption is warranted. The authors could refer to Figure 5 in this discussion. While the Hoalp catchment's Q95 trend appears to be linear, in the Buwe catchment the trend seems to be driven by a regime shift in the last 10 years of the record (most likely creating a trend in the residuals). The implication of this assumption should be discussed. For instance, are the error bounds associated with these projections impacted by this assumption? Is there a regime shift and not a linear trend, might you under-predict future low flows at this catchment?

We have added a note regarding the assumption earlier in the paper (where the linear trend model first appears), and we now address this point in the discussion section (referring to Figure 5), in particular the different shapes of the low flow changes in Hoalp and Buwe (trend vs regime shift). Regime shift is indeed a possibility and has now been given more prominence in the paper.

4) I believe the significance codes in Table 1 are incorrect. I think the symbols should either be switched in the table or in the table footnote.

Many thanks for pointing this out. The formatting error has been corrected.

5) A brief explanation of how groundwater discharge is modeled in the TUVmodel is warranted, as well as what parameters are calibrated in the SCE-UA routine.

A brief explanation has been added.

6) The results in Table 3 seem deceptive to me, since they are for model prediction across the entire streamflow regime. While the weights are changed to assess the impact of higher and lower streamflow prediction on Z_q , it's difficult to understand how these are important to this analysis. In addition, even though Table 3 says that this model does poorly at Buwe, the Q95 predictions in Figure 5 seem quite good. You might consider explaining this.

An explanation has been added.

7) There are a number of small typographic errors:

a) Page 13084 line 25. "(" before "Ceola" should be removed.

b) Page 13086 line 1. The "Q" in "ZQ" should be a subscript.

c) Page 13094 line 1. "on" should be "one".

d) Page 13097 line 7. "cam" should be "can".

e) Page 13099 line 16. "for Hundecha and Merz (2012)." should be "for (Hundecha and Merz, 2012)."

All these typos have been corrected.