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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modeling impacts of climate and land use changes on catchment hydrology: Meki River, Ethiopia" *by* D. Legesse et al.

Anonymous Referee #1

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This is an interesting paper, appropriate to the special issue focus. While the theme is timely and potentially applicable to most basins, this paper is lacking due to the limited analysis performed. The authors have engaged in a significant amount of work to establish the modeling system, yet carry out only a few sensitivity tests, thus making it difficult to draw any strong conclusions. The authors acknowledge that further analysis is necessary, and I believe that those results need to be added to this manuscript to make it meritorious. This includes additional temperature and precipitation change runs, joint changes to understand co-influences including land use, and addressing uncertainty of model parameters by drawing from parameter distributions (not just singly

optimized values.) In my opinion, this would greatly move this research forward to an interpretable state, useful for long-term basin decision-making. More details are provided in the following comments.

Additionally, I am not sure this manuscript is a clear advancement over the author's previous work in the Journal of Hydrology in its current state. Although it is an application to another catchment, practically the same sensitivity analyses (one change in P, one change in T) are undertaken. In that work, the author's advocate for expanding, however have not yet undertaken that here. The modeling system itself, presented previously, does not represent advancement, so the new contribution must come from any expanded analysis and additional conclusions.

Comments/suggestions:

Overall: grammar and wording could benefit from a careful check Overall: the term 'variability' is often used where 'change' appears appropriate, based on the focus of the manuscript (e.g. Page 4538, line 27.)

Page 4538, line 4: unclear how MMS and PRMS fit together, based on this description (MMS platform, PRMS model?). Better clarification needed.

Page 4538, line 18: better to define MMS first time in manuscript, not abstract

Page 4540, line 22: from description, sounds as if the outlet (Lake Z) not lowest point in the catchment. Is this true? Doesn't seem to match Fig. 1.

Page 4543, line 7: more details needed regarding filling in missing values (potentially very influential to overall interpretation.) How many are missing originally, how does the linear regression proceed, etc. Justification for this over other methods (i.e. why linear approach, not climatology or a nearest neighbor approach.)

Page 4545, line 2: should section 4.3 be moved to Section 2? More related to background.

Page 4546, line 3: how were the parameters identified through the daily sensitivity analysis? More details requested.

Page 4546, lines 15-21: the approach is slightly ad hoc (but not uncommon.) Is it possible to quantify the uncertainty in the parameters and therefore understand their ensuing influence? Perhaps a stochastic approach, drawing from parameter distributions, simulating over and over to realize the envelope of possibilities. If the uncertainties (and envelope) large, parameter (modeling) uncertainty may be greater than climate uncertainty, leaving minimal room of climate change interpretation.

Page 4546, lines 18-20: how does the auto calibration technique work (and if it's a common one, it should be named)? What is the objective value? Can multiple objective values be selected for a potentially more robust result? Can this technique provide uncertainty bounds or distribution is assist with the above comment?

Page 4546, lines 24-26: regarding volume differences, would this be better if a different objective value were used (e.g. root mean error)?

Page 4546, lines 27-28: limited in capturing peaks. . .good for dry seasons. Which is more critical for this catchment?

Page 4546, line 29: are additional metrics worth including that may emphasize other model characteristics?

Page 4547, line 1: very difficult to distinguish between observed and modeled in Figure 6.

Page 4547, line 3: "PRMS model simulated. . .well" based on what? Visual interpretation? Timing looks fine, although not possible to distinguish between days.

Page 4547, lines 3-9: an annual peak flow analysis could be insightful over the validation and calibration years: peak flow timing (e.g. histogram of days departing from observed peak) and peak flow quantity (e.g. scatter plot of observed and modeled.) If years are too few, could select top 5 events per year, for example.

Page 4547, line 11-13: any way to validate these separate contributions?

Page 4547, line 13: labels in Figure 7 don't match the text.

Page 4547, line 15-16: not surprising that monthly fares better than daily. This is common with temporal aggregation, and may be worth mentioning.

Page 4548, line 1-3: how do we know the parameters fit for the stationary (present) conditions apply to non-stationary conditions or a different climate state? This at least warrants some discussion.

Page 4548, line 4: results sections 4.5.1 and 4.5.2 feel small compared to lead-up text.

Page 4548, line 13: nine? How were the nine formed, and what do they represent? Also, how are P & T changes made (e.g. all P events boosted by 20%)? Need some justification that this is an appropriate assumption. Undertaking a range of P & T changes would be interesting to see relative effects, and where potential jumps or nonlinearities are, etc.

Page 4548, lines 20-end: suggest a table/matrix to display results.

Page 4549, line 1: impossible to visually interpret figures 9 & 10.

Page 4549, sections 4.5.1 and 4.5.2: adding compounding effects (potentially positive and negative feedbacks) between climate and land use scenarios would represent a strong contribution to this research. This is what will realistically be happening.

Page 4549, line 20: in this section, the first six paragraphs are actually a summary, so either need to drop or change heading.

Page 4550, line 26-end: understanding the influence of modeling uncertainties, especially in the context of climate and land changes, could be a major contribution of this paper. It puts the climate and land use changes in context by ultimately considering all uncertainty jointly to paint a realistic picture. Assessing independently is fine, but perhaps not at the exclusion of joint assessments. For some scenarios (e.g. low T or

P change), model uncertainty could overshadow climate changes. This, too, is useful information.

Page 4551, line 12-15: agree, and advocate for including it in this paper.

Page 4551, line 16-17: not sure that more GCMs contribute to a more “reliable” mean.

Page 4551, end section 5: may be worth discussing other demands on catchment water (populations, development, etc.) How do these relate, in terms of influence, to climate or land use changes, at least qualitatively?

Page 4551, end section 5: additional analysis could prompt stronger conclusions, which at present are a bit weak.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4535, 2010.