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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Identifying uncertainties in simulated streamflow from hydrologic model components for climate change impact assessments" *by* Dongmei Feng and Edward Beighley

Anonymous Referee #1

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General Comments (overall quality of paper):

Overall, I think the paper was well written as a climate impacts assessment and application of uncertainty methods provided elsewhere. As stated in the Introduction, the goals of the paper were threefold:

1) Compare different hydrologic models 2) Quantify uncertainty associated with different modeling choices 3) Provide suggestions for studies looking at impacts of climate change

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The paper accomplished these three goals (aside for one point which I address in the paragraph below). However, the authors don't make it clear how the field is moved forward even if all three goals are achieved. The authors' study appears to be similar to the Vetter et al (2015) study but in a different, and more homogeneous, domain. As-is, the authors conducted a very detailed assessment of climate change impacts on streamflow in Santa Barbara County. Their assignment of uncertainty to different modeling components followed methods similar to those in previous studies like Addor et al (2014), Vetter et al (2015), Hattermann et al (2018), Chegwidden et al (2019). I did not see any truly novel methods proposed, thus making the paper seem more like an, albeit very rigorous, report. As is, the study is appropriate for a climate impacts assessment journal, but the findings are insufficiently new to warrant publication in HESS.

To make the manuscript more relevant to HESS, I suggest a handful of other potential additions to deepen the analysis. Would it be possible to expand the analyses conducted here to other domains and thus do an intercomparison across different regions? For example, the findings in Figures 5 through 7 are relatively uniform across region and metric. Perhaps the authors could probe deeper into those comparisons by conducting more analyses in other regions or with other metrics? By expanding the analysis to other regions and metrics the study could test how sensitive the uncertainty analysis is to the research question of interest.

Another potential avenue of analysis could be a deeper understanding of the parameter space. I am skeptical about the finding that parameterizations explained little of the uncertainty since it appeared (from Figure 4) that the values within the different parameter sets evaluated were actually quite similar. Since it appears that you have those parameter sets available, would it be possible to expand the analysis to include more parameter sets? That could buy more confidence in the current analyses.

References not included in the current manuscript:

Addor, N., Rössler, O., Köplin, N., Huss, M., Weingartner, R., & Seibert, J. (2014). Robust changes and sources of uncertainty in the projected hydrological regimes of Swiss catchments. Water Resources Research, 50(10), 7541–7562. https://doi.org/10.1002/2014WR015549

Chegwidden, O. S., Nijssen, B., Rupp,D. E., Arnold, J. R., Clark, M. P.,Hamman, J. J., et al. (2019). How do modeling decisions affect the spread among hydrologic climate change projections? Exploring a large ensemble of simulations across a diversity of hydroclimates. Earth's Future,7,623–637. https://doi.org/10.1029/2018EF001047

Hattermann, F. F., Vetter, T., Breuer, L., Su, B., Daggupati, P., Donnelly, C., ... Krysnaova, V. (2018). Sources of uncertainty in hydrological climate impact assessment: A cross-scale study. Environmental Research Letters, 13(1). https://doi.org/10.1088/1748-9326/aa9938

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Specific Comments (individual scientific questions/issues):

L14- In the abstract, you mention that identification and uncertainties are rarely studied. This is not true. It is increasingly common (see, for example, the three references above).

L139-141 – Is the monthly 1 degree aerosol optical depth product sufficient for calculating radiation at the scale you are working at?

L154-156 – Should there be some discussion about the fact that, regardless of subbasin size (which, as the authors state, ranges between 0.1 and 135 km^2) the parameters are averaged across each subbasin?

L175-254 – I'm not sure the specificity is necessary for each of the hydrologic models in the main text. I would suggest moving the conceptual plot from the supplemental text to the main text and moving the mathematical explanations to the supplemental text. This would save you space in the main body of the text, improving readability, while letting your story come through easier. With that space you could fill in with more details on the calibration methods.

L260-263 – The definitions of Kss and Ks would probably best fit in the description of the routing model since they are from that model.

L268-269 – How are the three different optimal parameter sets selected? Are they very different parameter sets? As in, are they likely to be in very different parts of the overall calibration space/range of parameter values? Or are they likely to be relatively similar? If they are similar, does that explain the less than 1% uncertainty explained by parameterization referenced in L426 in the results? I see in Figure 4 that some of the parameter values for some models are indeed quite similar (e.g. Kss_all for RCM-HRR). How does this affect your conclusions about the minimal contribution of parameterization toward total uncertainty?

L320-324 – Do the authors conduct their performance weighting based upon the GCM simulations? Or do they do it using the historical meteorological forcing data (in this case Livneh et al)? The latter would be appropriate, since the former would not match the actual weather experienced by the region.

L360-363 – Is the climate data, even though it was downscaled to the 1/16th degree scale, appropriate for the scale of the subbasins the authors are evaluating (for instance the basin that is only 0.1 kmˆ2)? As the authors suggest in L360-363, they note substantial biases in the precipitation that, in one example case, doesn't even provide enough water to account for streamflow in absence of ET. Did the authors modify precipitation at all to account for this? If not, do the authors think that some other modification of the precipitation forcing would be appropriate? Also, in Figure 2 caption: (a) what does "normalized calibration process" mean?

L437-438 – "Changes in Qm, Qp and Q100 are higher under RCP 8.5, but the uncertainties are also higher (Fig. 8), which suggests the uncertainties from RCPs are mainly introduced by RCP 8.5." Could you clarify this statement? I think there may

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be some conflation in the sources of the uncertainties in this argument. In looking at Figure 8, we see that the distributions are very different between RCP45 and RCP85. However, in your ANOVA formulation, the comparison of the different model choices really just looks at the differences in the means. Thus, attributing the uncertainty to RCP 8.5 can't be made by these figures alone, since you are only comparing two choices. If you are referring to the large standard deviation of the RCP85 distribution, then that uncertainty contribution would actually be a higher-order interaction of RCP and something else (perhaps GCM?).

L394-396 – I assume the reference to the National Land Data Assimilation System VIC model set-up is the one referenced at the following DOI? (https://doi.org/10.5067/ELBDAPAKNGJ9) If so, it needs a citation and perhaps some explanation as to why this is used as a suitable comparison.

L446-449/456-457 – How can you justify that model configurations (e.g. irregular catchments or routing schemes) are the reason that hydrologic models played a smaller role in your uncertainty findings?

L449-451 – What do the authors mean by "a common calibration approach is also used to eliminate user/method bias"?

L461-462 – Is reducing the uncertainty the goal for an impacts assessment? Would not the goal actually be to reveal the uncertainty present, and thus actually focus on multiple hydrologic models as the authors suggest that their selection accounts for a sizeable portion of the uncertainty space?

L471-475 – At the relatively small scale which you are working, how is routing impactful?

L483 – How do you define uncertainty of 230%? Is that the range? Or +/- 2 standard deviations?

================================ Technical Corrections (typing errors,

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etc):

L43 – “cause” not “causes”

L69-70, L81 – Confusing sentences/phrasing

L220 – “matric” not “metric” – there are many other language typos (e.g. L222 “expresses” should be “expressed”) sprinkled throughout the text, but I imagine that with another read-through these issues could be resolved.

Overall, there are small language errors throughout the manuscript which the vast majority of the time don’t interfere with understanding but are somewhat distracting. A careful reading would help resolve these.

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