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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Future shift of the relative roles of precipitation and temperature in controlling annual runoff in the conterminous United States" *by* K. Duan et al.

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The authors have provided a scientifically sound manuscript documenting the attributable effects of precipitation and temperature on runoff in future climate projections. It is, indeed, an interesting contribution to the literature, sparking a valuable discussion. However, I believe that some additional description may improve the manuscript. My comments are presented in no particular order.

Due to the unique nature of reviews in HESS, I have had the opportunity to review the comments of the previous reviewer and the authors; proposed response. For this reason, I will not retread any of the same ground except to say that I whole-heartedly agree with his or her review. My review to will take off from the assumption that the

concerns of the previous reviewer have been wholly addressed.

Firstly, I would ask that the authors provide a little more support for their selection of climate scenarios. They state that the scenarios selected represent a medium and a high emissions scenario. While I am supportive of this approach given the current state of emissions, I feel the authors should provide objective reasoning for not selecting a lower emissions scenario. Providing objective reasoning will more strongly avoid accusations of bias selection.

I was happy to see that authors' discussion of precipitation and model uncertainty. However, I feel this is a very important issue and could use additional discussion. As the previous reviewer observed, it can have a dramatic effect on the interpretation of the results as well as the application of statistical hypothesis testing. Do the current results and understanding of uncertainty allow you to posit anything about the informal significance of these changes? How might decreased uncertainty improve accuracy?

Finally, related to the question of uncertainty, I would like the authors to discuss the partitioning of attribution a little more fully. As the previous reviewer has observed, P and T are not treated as independent forcing in this work. Could you provide some additional discussion on this point? More importantly, I found myself looking for a justification on how these could be separated. For example, what changes in P are not derived from changes in T, whether directly or indirectly? It seems that any change in runoff due to precipitation may be indirectly interpreted as a change induced by T. While I do not disagree with the authors premise, I feel it should be more clearly articulated.

Again, not retreading the ground of the previous reviewer, I will leave my review here. I have not conducted an editorial review of the work, though I did notice, as did the previous reviewer, that some editorial work is needed before publication; I am sure that HESS will provide this resource. In addition to the comments of the previous review, I would ask that the authors pay particular attention to statistical testing. For example, the null hypothesis of a test should always be stated with an alternative hypothesis.

Additionally, the results of a test should never be reported exclusively as “significant” or “not significant”. Given the wide availability of computational software, this determination should always be coupled with a reported p-value, allowing a true assessment of the strength of the evidence. Binary significance is no longer appropriate in scientific literature. In a similar vein, it may behoove the authors to refer to climate change rather than global warming, the former being a more accurate representation of the situation.

Thank for the opportunity to review this work. It was well executed and I look forward to continued discussion.

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