

# ***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.***

## **Anonymous Referee #1**

Received and published: 22 November 2016

This paper has the potential to be an interesting contribution to the literature, as the changing roles of temperature and precipitation under a changing climate are of both scientific and practical importance. I did find a number of places within the text that additional information is needed to properly assess this work.

1. I believe this paper quite nicely follows the paper by McCabe and Wolock [2011] that is cited throughout. Specifically, the last sentence of that 2011 paper states, “If temperatures continue to increase, as projected by climate models, the effects of temperature on runoff may become more apparent.” I think it is important to add to the introduction that this manuscript appears to be the first such contribution that looks at the relative contributions of P and T under future climate scenarios. It should also

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be worth noting in the introduction that the joint roles of P and T are examined here and not treated as independent forcings, as was done by McCabe and Wolock [2011]. Adding these points would help to draw context to this work and provide a further case for its relevance in the literature.

2. p. 4, line 22: The statement is made “although the possible underestimation of the influence of temperature. . .has been discussed in recent researches” but only one citation is offered with an “e.g.” - either there is only one paper that has discussed this or there are more. If there are more, they should also be cited. If there is only this one paper, the “e.g.” should be removed. If this was a recent discussion in the literature, then there should not be a reason to only cite a sample of papers - simply cite them all. This also lends credence to the manuscript.

3. p. 6, lines 10-12: Here it is noted that 10 different land-cover classes are used to account for water demands. Later the authors note that changing land conditions are not considered. To avoid confusion, I would add this limitation here in addition to later in the text. Something like, “Whereas land use is included in the assessment of water demand, the effects of changing land use are not.” I know this point is noted on line 19 but perhaps moving that up to line 12 would make this point clearer.

4. p. 6, line 20: Please explain how the monthly climate inputs were scaled to the HUC-12 watersheds.

5. p. 7, line 6: The ability to estimate ET well seems an important component to the validity of the results. Are these parameters static in the model, as the manuscript notes that are estimated empirically? It would seem that the ET parameters would adjust under future climate conditions. If they are static, could this bias the estimates of the relative effects of precipitation and temperature under future conditions?

6. p. 7, lines 20-21: The R-squared values for only the highest values are reported and not for the all results. This is a bit misleading; the reader cannot assess the performance across the entire CONUS and WRR scales. When looking at figure 2, the

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captions need to also show the number of data points in each plot. It is unclear why the WRR plots have a different number of points than the CONUS plots and this is not well-explained in the text. There is no mention of the number of gauges used on the validation process. This is a concerning oversight that must be addressed and further reviewed.

Equation 1: Consider writing in terms of R so that the reader can look at the P and T terms on one side of the equation.

p. 8, lines 5-7: I was left wondering if the ET is oversimplified to the point that it does not consider the non-linearity and feedbacks in the climate system. Could the authors comment on this?

p. 11, Section 3.1: Cite the corresponding evidence (figures) for these observations.

p. 12, lines 17-19: What are these observations based on? I think the figures are created based on equation 2 but please add this detail here.

p. 13, line 1: GCMs do not treat changes in temperature and precipitation separately, correct? So why is the word independent used here. Please clarify.

p. 14, Use of the Wilcoxon signed rank test: I believe more detail is needed here. What are the sample sizes used in the test - is it N=20 for the number of GCMs per region? More details about the test and the assumptions are needed. This statistical test assumes that the data come from the same distribution, which may not be the case if the 20 data points come from 20 different GCM results. Furthermore, on p. 14, line 22, are these differences significant? An increase in T from 60 to 63 percent does not seem that would be significant. Lastly, the Wilcoxon test does not account for the substantial variability inherent in each of these estimated changes - variability from the GCM output, the downscaling, etc. I would think there would be much less of change to find any significant differences if the actual variability of the estimated changes were included.

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The authors should be commended for the discussion section, which helps to address the limitations of their analysis and place appropriate qualifiers on the interpretation of the results. Nicely done.

Editorial comments:

There are numerous places where there is awkward wording or pronouns missing. I was able to catch some of them in my reading but more still exist in the manuscript. Please do a thorough read-through to ensure the manuscript addresses this.

p. 3, line 11: Change “the” to “a”

p. 3, line 21: Change “results” to “result” and “decrease” to “decreases”

p. 4, line 4: The authors use climate change throughout. Change “global warming” to “climate change”

p. 4, line 12: Change to read “under a warming climate”

p. 4, line 15: Change to “CONUS has reached”

p. 4, line 22: Change to “has been discussed recently”

p. 5, line 6: Delete “the state of the art”

p. 5, lines 6-8: Delete the sentence that starts “In another word...” This is confusing and does not add anything.

p. 5, line 9: Should this read, “quantify the relative contributions of...”

p. 5, line 12: After “spatial pattern of runoff change” add over what period you are examining the change.

p. 6, line 7 (and throughout): HESS guidelines are very clear about how to abbreviate evapotranspiration and how to reference other variables. The authors should review these practices and adjust accordingly. See more information below.

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Specifically (from [http://www.hydrology-and-earth-system-sciences.net/for\\_authors/manuscript\\_preparation.html](http://www.hydrology-and-earth-system-sciences.net/for_authors/manuscript_preparation.html)):

“Multi-letter variables should be avoided. Instead use single-letter variables with subscript (e.g. ERMS instead of RMSE, or ET instead of ET). Single-letter variables or parameters and user-defined function symbols should be italic (e.g.  $x$ ,  $Y$ ,  $\beta$ ,  $f(x)$ ). Multi-letter variables, if they cannot be avoided, should be roman.”

p. 14, line 15: What is “it” in this sentence?

p. 18, line 2: Precipitation, temperature and runoff should be abbreviated to be consistent with your use of these terms elsewhere in the discussion section and throughout the manuscript.

Tables and figures should be stand alone. In the tables, only S1-S4 are referred to as scenarios with no other explanation. These scenarios should be explained on each table. Abbreviations should be spelled out (see examples in figure 1, CONUS as an example where this is not done; figure 2, R and P are used in the figures but not explained in the caption.)

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