Review of
"Climate Regime and Soil Storage Capacity Interact to Effect Evapotranspiration in Western United States Mountain Catchments"
by
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Main comments

This manuscript reports on a study of the sensitivity of Evapotranspiration (ET) to interannual variations in Precipitation (P), temperature (T) and soil recharge (R_{75}) at three catchments in the western United States. The study is based on the analysis of the results from hydrologic simulations using the hydro-ecological model RHESSys (Regional Hydro-Ecological Simulation System). Additionally, a sensitivity analysis is performed using variations in the statistical relationships between P, T, and R_{75}, and ET (the dependent variable) for varying soil characteristics introduced during the calibration procedure of the model. The calibration consists of 800 Montecarlo-type runs of the models in which soil parameters are selected from a uniform distribution of realistic values of the soil parameters selected to be within realistic ranges for the types of soils found in the watersheds (based on literature). These parameters are used to calculate a parameter called AWC (soil water holding capacity) that represents the capacity of the soils in the watersheds to retain/store water, calculated as the difference between the wilting point and the field capacity. The authors discus the results from these sensitivity analyses and presents some explanations for such results.

In general, the article has a number of unexplained assumptions, and it lacks details required to support the validity of the analyses. I point out several of these issues in the detailed comments below. Also, the title implies that somehow the three basins studied here are representative of what would be observed across the western US, which is inaccurate, or at east, is not really proven in the manuscript.

In reality, the authors perform sensitivity analyses to some of the driving variables of the RHESSys model in the three basins. However, the results are presented as a conceptual model of ET in response to climate and geologic characteristics of the basins. The conceptual model (section 2.1) is rather a discussion of the variables that the authors expect to be the principal controls of ET in the catchments. This is not really a model and should be clarified. I was confused reading through the manuscript as ultimately there is no other model other that RHESSys.

The variables compared to ET include P, T, and R_{75}. There is no explanation in the text as of how these variables are calculated, are they averaged over the catchment for each water year? What are the ranges of the variables within these catchments? Large variations in these parameters should be expected in the catchments given the variations in topography, relief, soil properties, vegetation cover, and weather/climate. However, none of this is discussed.

There is also little in the manuscript about the model implementation in the study basins. The authors only briefly discuss the model implementation in one of the catchments, and refer the readers to two other publications (one for

which the reference is missing in the list). The model implementation and assumptions are important for the readers to understand the model's capability to accurately capture the processes that influence the variables analyzed, as well as the uncertainty expected in the model's output. This is not really discussed or presented in the manuscript.

In summary, the article needs major revisions, the descriptions of the simulations and how the parameters are obtained needs to be expanded, and the conclusions from the analyses need to be stronger. In the present form, it is hard to really assess what the contributions of the study really are. After multiple readings, I gather that the authors show that ET response primarily to P, followed by T, and R_{75}. The additional sensitivity analyses of the slope of the scatter plots between all these variables as a function of AWC complements these results. However, these do not represent a significant contribution to the current knowledge. Overall, as a reader, I was left with enough questions about how the results are in reality representative of the processes in the watersheds.

Detailed Comments
In general the use of the term "snowpack" is odd. Please review throughout the text. I included a few examples in the detailed comments but there are many others in the text.
The normalization of some variables needs to be explained. Normalized by what? (I deduct the mean from the text, but this needs to be explained)
A conclusion section should be added
p. 2278.
ll. 11-12. "As we expect" could be eliminated.
p. 2279
l. 17. "Snowpack magnitude" does not seems correct. Perhaps, "maximum snow accumulations" or "Peak SWE" would be more descriptive.
l. 23. Again, "peak snowpack" does not refer to a quantity.
p. 2281
l. 15. Same as above: "magnitude of snowpack". Review throughout the manuscript.
p. 2282
l. 13. "all" is repeated.
ll. 14-15. By which mechanism does climate alter water demand? Explain, the end of the line leaves this still unclear.
l. 16. Which geologic and topographic properties? Expand.
l. 20. "spring" or "winter and spring" rather than just winter?
l. 26. Clarify if "winter precipitation" is referring to snow,
l.28. "then soil will have little availability to extend water availability" reads odd to me.
ll. 10- p. 2283. l.9. There are no references in this paragraph, however, there are a lot of processed mentioned without support.
p. 2283.
l. 3. "uncontroversial" sounds odd here.
p. 2285.
l. 9. What statistics were used?
p. 2286

ll.1-2. The statement about the future research is unnecessary.

l. 7. What "statistical analysis"? The term is too vague.

ll.7-16. I would like to see the analysis, e.g., the scatter plots. I think the reader would wonder about how these relationships look like. Also, What temperatures are being used here? From single stations? At what elevations? Are they representative of the whole basin? Generally speaking, this is is hardly the case, for which you should provide more information to understand the limitations of the data used here.

ll.12-13. Include the correlation values obtained.

l. 18. Which water limited basins? Clarify. Also, I don't see how the authors have tested the water limitation in this basins, how is this known? No support is included.

l. 23. I don't have this clear. Do you need to know the total recharge to know what the 75% would be? How is the total recharge estimated? And, from reading this, it seems like the assumption is that all water that reaches the soil infiltrates, which is not correct depending on many factors such as infiltration rates, soil properties, saturation, water fluxes reaching the surface, etc.

l. 24. A comma is missing after "rain".

ll. 24-26. What is the justification for this assumption? It should be included in the text.

p. 2287.

ll. 15-17. Does this mean that there is only one soil type or an average per watershed?

l. 21. 25-45 yrs refers to what period? Include

p. 2288.

l. 19. Can the Colorado basin really be considered Mediterranean? Also, an explanation on what a Mediterranean climate is pertinent here.

l. 24. The word "loss" does not seem appropriate here. And how is this known? How about soil recharge, etc?

l. 28. It may be more clear to expand "Carbone" and "Nitrogen" in this line.

p. 2289.

ll. 4-5. Garcia et al. (2013) is missing in the reference list. Tague et al. (2013) is Tague and Peng (2013)?

l. 12. No need for the word "downloaded". Also, opening "(" is missing before the link.

l. 14. Is the date of access needed? I'm missing the point of some of the information in these lines.

l.15. "so" can be eliminated

l. 20-21. A comma is needed after "rock". Same comment as above (l. 14).

ll. 26-28. According to these lines, only two points (Stations?) are used to estimate the spatial distribution of temperature and precipitation that drives the model for this 350 km^2 basin. This must lead to a lot of uncertainty in your results.

l. 27. How is this shift determined at this elevation? And what does climatic regime refers to?

pp. 2289-2290.

P. 2290. Here, only the model of CO-ROCK is described, yet the manuscript uses the model results for three basins. I understand that this is done because the models for the other two basins are presented in the two references in the

paragraph before, but what it really means is that I (and the readers) have to read two more papers to really get an idea of what the other model implementations required. The article should be comprehensive, even if the support for the models of the other two basins is included in the other articles. At least a summary of the implementation of the model for the other two basins should be included here; I was lost trying to figure out why only one basin was included. Also, the first reference of the other two basins is missing in the reference list.

l. 8. Opening parenthesis missing.

l. 20. This is a writing style comment, but statements such as "not surprisingly" in this kind of sentence seems unnecessary.

l. 21. Significant as in statistically significant? Or how is the significance evaluated? A correlation value and p-value?

l. 26. I suggest using "dryer years" instead of "dry years"

p. 2291.

l. 1. Include the p-value or other statistics used to evaluate the "significance" of the relationships. Check this throughout the test and reword when appropriate.

ll.5-6. The water-limitation of the basin can not simply be assessed by the percentage of precipitation during the growing season. This should include more rigorous support.

l.7. "in all watersheds". In is missing in text.

p. 2292.

l. 7. "lost" again, see comment above. Also, I don't see any justification for this statement. If the meltout is earlier, how is the infiltration and net recharged reduced? For example, if the same amount of snow is melted, how does that lead to an increase in runoff and a decrease in recharge just from having earlier melt?

p. 2292.

l. 25. Expand on the methodology by Muggeo.

p. 2293.

ll. 15-18. Include TWI values.

l. 22. Normalized by?

p. 2295-2296

ll. 25-28. This sentence is too long and hard to follow, rephrase.

p. 2297.

ll. 7-10. The sentence needs rewriting, and "isn't" should not be in a scientific text.

l. 13. "Snowpack acts as a storage mechanism" is odd. Reword. Also, using the active voice for an inanimate object may not be the best way to describe this. But again, this is a writing style comment.

ll. 13-17. This sentence does not really present much information. For example, how is the timing of soil moisture recharge and snowpack dynamics "important"?

l. 18. Again, "important"

ll. 20. "rather than" not "then"

l. 24. "modest" is vague, what does it represent?

The location of the basins should go in one figure. The insets are too small to clearly see the location.

Figure 2. Insets and text is hard to read. Same as lat. and long. Coordinates.

Where are these coming from? Are they at a single station or are the averaged over gridcells or patches of the basin? How is the data displayed obtained?
The captions should present/describe what is in the figures (e.g., fig. 4). Also, you can leave the main message or statement that the figure conveys in the text, not needed in the figure. Also, in general, you can use smaller markers to reduce overlap.