

Interactive comment on “Using hydrologic landscape classification and climatic time series to assess hydrologic vulnerability of the Western U.S. to climate” by Chas E. Jones et al.

Anonymous Referee #2

Received and published: 6 April 2020

This paper aims to apply the hydrologic landscape approach to chronicling changes to the western US under climate change. Using a vulnerability index, the authors aim to highlight locations more or less prone to changes in climate for various indices. It is apparent that the manuscript was a technically challenging effort to reconcile multiple datasets and climate scenarios and synthesize them in a GIS framework. The work there should not be discounted. The manuscript is generally well written, but feels disjointed and an attempt to reconcile several disparate research efforts between a discussion on climate change, hydrologic landscapes, and vulnerability of socially/economically valuable locations. I understand what the authors are aiming for if the authors aim to show all three aspects, they could be better unified. Overall,

I think this paper is worthy of publication and the data analysis is commendable, but better structure and explanation is needed. I recommend the paper be revised and resubmitted.

Specific Comments: L64-65: "The findings are consistent across studies in many areas of the globe...." –How are they consistent across studies and which studies? And the second half of this sentence seems to contradict the first half when you say they aren't consistent without any citations being given. Which one is it?

L175-177: The methods describe that Leibowitz et al (2016) used a modification of Wigington et al (2013). It's a little unclear as to what the modification was. More clarity or explanation needed here.

Section 2.3: I'm a little unclear as to the selection of these dates and selection of data. –Why is 1971-2000 considered the modern climate normal when such data is at least 20 years ago? It seems incongruous to have this be your "modern" normal when you consider "historical" data to be up to at least 2010 and state that the PRISM data you use for your calculation of modern normals goes from 1895-present. Why not have the modern normal represent a more recent time period? –I'm also unclear as to why monthly precipitation and mean temperature data is acceptable for the modern climate normal calculation (L230-232), but daily measurements are needed for the historical decadal analyses (L240) and they're subsequently averaged to monthly means anyways. The requirement for daily data caused you to employ a downscaling approach, potentially introducing more error. More explanation is needed here.

Section 2.3.3: Better explanation is needed as to what were the criteria for choosing the 10 modeled emissions scenarios. Figure 2 appropriately shows their distribution in terms of precipitation and temperature, but how were the 10 out of the at least 38 chosen? Random draw? Some other selection criteria? Further, the coloring and subscript numbers in Figure 2 needs to be better explained in the caption.

Section 2.5: Better explanation is needed for how these sites were selected and how

[Printer-friendly version](#)

[Discussion paper](#)



their areal extent was decided (see Table 2). Site specific areal extent appears to range from 38 km² to 4855 km² (Table 2). Also according to section 2.2 the target AU size is 80 km² meaning that at the low end (e.g. Great Basin NP w/ area of 38 km²) is likely composed of a single AU and many only a few units. I get the challenges of making AUs a representative size across multiple different spatial datasets, but some discussion of how that AU size and differential location areal extent affects these location based analyses is warranted.

L312-313: "The time series for the decadal averages for each of the seven HL metrics..." I think you mean to say the seven climate related HL metrics here because things like elevation, subsurface permeability, and surface permeability aren't subject to change under this approach.

L325-327: The sentence beginning "In terms of the 1971-2000 climate normal period" needs some revision. I think it needs a clause saying, "followed by 24% of the area showing fall seasonality, 13% spring seasonality,...."

L342-343: Needs some clarification. What remaining models? You said in methods you only tested 10 and in the preceding text you said 3 may be wetter and 7 generally drier. What models are left?

–Several times in the results and discussion you point out patterns shown along major geographic features like mountain ranges (just as one example paragraph beginning on L351). It would be beneficial to show where those are like you do in the ecoregions in Figure 1. Many of the readers may be unfamiliar with where these features are so it makes it difficult to place the patterns you're describing.

–Several times the authors talk about the sensitivity or vulnerability changes without talking about the direction of that change (but see sentence beginning on L371 "The map for S" as an example). It'd be useful to make sure if you're saying an area is vulnerable to a change in climate, it's not just the metric (e.g. temperature) but also whether higher/lower or earlier/later.

[Printer-friendly version](#)

[Discussion paper](#)



L385-392: I mentioned in my introduction that the paper was one that seemed disjointed between the paper being primarily one communicating climatic changes with a discussion of HLs and vulnerability thrown into the mix. This is an example of where the authors do a good job of uniting at least the HL approach with the climate indices. The HL approach is the story of the complete code where certain indices may play a more proximal role in given locations. This section does a good job of explaining how the changes in the climate indices could have differential effects based on things like elevation or the permeability metrics. More discussion like this is needed throughout the paper.

Figure 3-4. These colors are hard to discern with much of the area looking a yellow color that according to the scale is no change. I wonder if more of a categorical variable would be appropriate here to show changes rather than a color ramp. The reddish hue is more noticeable in Figure 4 for sure, but I wonder if this could be better communicated.

Figure 7. I wonder if here too classed variables may be better used to show variability by placing them in a low, medium, high type construct rather than a color ramp.

Figure 8. I'm confused by several aspects of this figure. –Several of the figure panels don't have corresponding descriptions in the caption. For instance there's a two separate panels for April 1 SWE and Snow, but the figure caption says "snow (April 1 SWE (mm))" as if they're combined somehow. This is confusing especially when the "Snow" panel has a y-axis labeled 1-2000 without any units. –Also the panel labeled "Climate" I believe is referenced as "FMI" in the caption. Also on this panel the left y-axis is from -1 to 1 while the right axis is the categorical Arid-Very wet labels. This needs to be better explained as it's confusing even to someone familiar with HLs. –Finally, the climate projection section is also confusing as sometimes it appears there's two lines while others there are several (e.g. Mt. Hood SWE panel). It may be cleaner to just show the high and low range lines rather than all the model scenarios I think you're showing. What the gray shaded area is showing also needs to be described in the caption.

Other Discussion Points –The discussion of the site specific locations seems a bit disjointed in the discussion. I wonder if it'd be better served to be called out as case studies in a subsection. The discussion seems to go big picture, dive down to case studies, and then back out to a discussion of HLs. This organization seems a bit haphazard and cobbled together. I wonder if better flow and cohesiveness from section to section could be achieved here.

–The results section is dominated by description of the climatic time series and changes to the HL indices classifications, but explanation of those changes largely disappear in the discussion and is dominated by discussion of vulnerability. I get that the vulnerability index is an attempt to merge some of those ideas, but I would have expected better mixing of the climate and HL information in with the vulnerability discussion.

–You stated in the methods that you chose from the highest emissions scenarios climate data projections (RCP 8.5). Better admonition of that fact needs to be detailed in the discussion as several other projection scenarios show lower degrees of change or better explanation of why you thought the high-end emissions scenarios were most representative needs to be explained.

-I think there could be better discussion as to how having high vulnerability in a single metric could have profound implications in some areas while other areas may only be affected by having high vulnerability across multiple metrics. You get at some of this in the case study approach where certain grape varieties are more impacted by temperature changes say rather than precipitation changes, but I think that could be expressed better throughout including in the discussion of HLs. For instance a change in seasonality could have profound implications to overall hydrology if that change meant a state transition from snow to rain even with a relatively modest change in temperature. There's a robust literature (especially for the west coast) on the impacts of these projected changes. Maybe some incorporation of overall vulnerability across all these indices is warranted. Surely that's industry or stakeholder specific in what they deem

[Printer-friendly version](#)

[Discussion paper](#)



"important" as highlighted in the case studies, but better discussion here may be warranted.

—Along those same lines, you dedicate a lot of space both in terms of figures and text towards changes in seasonality (Figure 4-5) and FMI (Figures 2-3). Some discussion on whether you expect those to be the most consequential HL metrics in this region would be useful.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-638>, 2020.

Printer-friendly version

Discussion paper

