

## ***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.***

**Chas E. Jones et al.**

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Thank you for offering us the opportunity to respond to your (RC2) comments and feedback on our manuscript titled “Using hydrologic landscape classification and climatic time series to assess hydrologic vulnerability of the Western U.S. to climate”. We believe that we can effectively address the specific concerns in an additional revision that will improve the manuscript and make it more worthy of publication in HESS. We found the reviewer feedback to be insightful and are certain that it will ultimately benefit the manuscript.

RC2 Comment 1) This paper aims to apply the hydrologic landscape approach to

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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.

Author Response 1) Thank you for this feedback and recognition of the value of the research effort. RC2 recognizes that a strength of the manuscript is our attempt to integrate three disparate fields of 1) climate change, 2) Hydrologic Landscape classification, and 3) the socioeconomic impacts of climate change. We propose that we can highlight this strength of the analysis in the introduction which will also help us unify these aspects of the study. We propose to add some language of this unifying concept into the Introduction and then revisit the unifying concept in the closing paragraphs of the Discussion or Conclusion sections.

Specific Comment 2) (Lines 64-65): "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?

Author Response 2) This sentence is referring to McAfee's 2013 study and was intended to summarize her findings. We will modify the text to clarify the study's results.

Specific Comment 3 (Line 175-177): The methods describe that Leibowitz et al (2016)

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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.

Author Response 3) We can add text to further summarize Leibowitz et al. (2016)'s modification to the Wigington et al. (2013) methods.

Specific Comment 4 (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?

Author Response 4) We agree that the use of the term "modern" is inaccurate and we will remove it. We chose to use the 1971-2000 period because the analysis was intended to complement the Leibowitz et al. 2016 study, which used 1971-2000 as its defined "climate normal". We can add explanatory text to our reasoning for defining our normal climate period as 1971-2000.

Specific Comment 5 (Section 2.3; Lines 230-240): 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.

Author Response 5) While we alluded to this detail on line 241 in the original manuscript, we had previously acquired the 1971-2000 400m monthly climate normals for a fee. Budget constraints did not allow us to purchase the remaining decadal data at 400m monthly resolutions. However, the 4km resolution daily historical data was available at no cost. Therefore, we chose to utilize the freely available data in our analyses. The daily 4km data were used to generate mean monthly gridded datasets and

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were downscaled to match the scale and resolution of the 400m mean monthly precipitation and temperature datasets. While we expected that the daily data would have greater accuracy overall, we felt that both datasets should be comparable at monthly time-scales. We can add explanatory text that clarifies the reasons for these decisions.

Specific Comment 6 (Lines 262-265 and Fig. 2): 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.

Author Response 6) We can add clarifying language to lines 262-265 regarding the model simulation selection process. We can also add clarifying information about the figure coloring and naming conventions to the caption of Fig. 2.

Specific Comment 7 (Section 2.5; Lines 309-316): Better explanation is needed for how these sites were selected and how their areal extent was decided (see Table 2). Site specific areal extent appears to range from 38 km<sup>2</sup> to 4855 km<sup>2</sup> (Table 2). Also according to section 2.2 the target AU size is 80 km<sup>2</sup> meaning that at the low end (e.g. Great Basin NP w/ area of 38 km<sup>2</sup>) 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.

Author Response 7) Specific focus sites were selected subjectively so that we could examine climate impacts at locations that may be of general interest. In addition, the range of Assessment Unit (AU) areas represents watersheds that are larger than hill-slopes but smaller than large basins. We can add explanatory language to explain that background information. We can also explain that all of the AUs that had greater than 50% of their area within the geographic boundary of a location were included in the AU

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analysis for each location. For instance, the Great Basin National Park (GBNP) was covered by a single AU, rather than numerous AUs because less than 49% of the area contained by the other AU areas fell within the GBNP boundary.

Specific Comment 8 (Lines 312-313): "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.

Author Response 8) Good catch. We will add the 'climate-related' descriptor to our reference to the seven HL metrics.

Specific Comment 9 (Lines 325-327): 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,."

Author Response 9) Absolutely. That is an awkward sentence that needs to be revised. Thank you for noting that the sentence structure needs to be improved.

Specific Comment 10 (Lines 342-343): 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?

Author Response 10) Thank you for noticing that duplication of information that basically repeated the information with different wording. We will delete the duplicative text (L340-343).

Specific Comment 11 (Line 355): 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.

Author Response 11) In this paragraph, we do reference the White Mountains (L355),

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which is Location #42 in Fig. 1 and table 2. In any case that we have referenced place names that correlate to Locations identified in Table 2 or Fig. 1, we can reference the assigned Location # as well. We also refer to the “Sierra-Nevada Mountains”, “Cascade Mountains”, and “Mountainous areas in Idaho”. We could also address this familiarity issue by either deleting these references in the manuscript; adding a supplemental figure; or adding additional information to Figure 1.

Specific Comment 12 (Line 371): 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.

Author Response 12) While it is possible to talk about direction of change (higher or lower than the two standard deviations) for the projection of an individual climate model, the vulnerability index is the integration of ten individual models. It is possible for individual models to exceed the two standard deviation threshold from the mean in both the upper and lower directions; thus there is not a unique direction of change associated with our vulnerability index as we've defined it. We can add text to the methods and results that clarifies this detail of our Vulnerability Index.

Specific Comment 13 (Line 385-392): 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.

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Author Response 13) Thank you for pointing out that the integration of the HL approach with the climate indices is a unique aspect of our manuscript and is worth expanding upon. We can add more text or otherwise add additional HL context to our discussion of climate and the associated socio-economic implications to the introduction, discussion, and/or conclusions.

Specific Comment 14 (Figures 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.

Author Response 14) Figures 3 and 4 do illustrate the actual geographic differences in FMI across large regions. When mapping the differences categorically, the differences either appear exaggerated or absent. Thus, we would prefer to retain these figures in their current form, as we consider categorical differences to be less inaccurate and possibly misleading.

Specific Comment 15 (Figure 7.1): 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.

Author Response 15) These images do depict classified variable (10 classes), however, the legend suggests that this is a continuous variable. We will modify the legend to clarify that this depicts a classified variable. We would prefer to retain the figures in their current form, as we consider that depicting vulnerability into fewer classes would be less accurate and possibly misleading.

Specific Comment 16 (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

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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.

Author Response 16) Thank you for the attention to detail. There was an error in the labeling of one of the figures. The "Snow" figures should be labeled as "PET". We can add clarification to the Climate / FMI panel of figures. We will also explore removing the red dashed lines that illustrate the individual climate model outputs, and only including the range of projections.

Specific Comment 17 (Lines 426-521): 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.

Author Response 17) We can add a subsection to the discussion for case studies and make the discussion section more cohesive.

Specific Comment 18 (Sections 4 and 5: Results and Discussion): 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

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climate and HL information in with the vulnerability discussion.

Author Response 18) We believe that some of the above proposed changes will address this issue in a revised version. We propose to further highlight the integration of climate, the HL classification approach, climate vulnerability, and socio-economic impacts throughout the paper, especially the intro, discussion, and conclusions.

Specific Comment 19 (Lines 260-262): 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.

Author Response 19) Good idea. We can add emphasis to the discussion so that it is clear why we chose to analyze and present only results and implications that relate to the RCP 8.5 pathway.

Specific Comment 20 (Other): 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 "important" as highlighted in the case studies, but better discussion here may be warranted.

Author Response 20) We can add further discussion about the implications of elevated

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vulnerability in a revision. This, combined with some of the previously proposed revisions, would strengthen the manuscript.

Specific Comment 21 (Section 4: Discussion): 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.

Author Response 21) We can further discuss changes in seasonality and FMI in a revision. These two metrics integrate numerous aspects of climate change into a single metric, and tend to be metrics that are of general interest. Thank you for pointing out a potential imbalance in our analyses.

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