

Interactive comment on “Hydrologic landscape classification assesses streamflow vulnerability to climate change in Oregon, USA” by S. G. Leibowitz et al.

Anonymous Referee #2

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Overall comments: This is an interesting approach to try to depict modeled future climate change effects on hydrology in Oregon. However, the paper draws heavily on previous work and it is difficult to understand the current analyses without reference to these earlier papers.

Major issues:

1. Title and abstract are misleading, because paper does not clearly define or use vulnerability assessments, and there is a strong focus on salmonids not reflected in the title.

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2. Paper argues that the classification approach is distinctive from analysis of historical data or use of models, but does not make the argument convincingly.

3. The outcomes depend heavily on a calculated S value which appears to represent water available for runoff, yet this value does not match observed runoff and this issue is not addressed in the paper.

4. The main conclusions from the three case studies refer to aspects of groundwater, geology, and soils but these aspects of the classification system were not described or defined in this paper.

5. The choice of only 2 GCM models is not justified in terms relevant to the study question.

6. Results are pretty much what we already know from GCMs directly - what are we learning?

7. Tables and figures need more explanation.

Detailed comments

The title of the paper is misleading. "Hydrologic landscape classification assesses streamflow vulnerability to climate change in Oregon, USA" - A vulnerability assessment strictly speaking is not conducted, and the contents of the manuscript focus on three case study basins as well as

p. 2879, l. 10-23. Vulnerability is mentioned here, but not defined. This study does not seem to qualify as a vulnerability assessment, because it does not address exposure, sensitivity, and adaptation. The manuscript contains a lot of discussion about salmonids. Maybe salmonid vulnerability is what the authors were concentrating on? But this does not come across clearly. Needs tightening.

p. 2879, l. 16 onward. This section makes an argument that classification (as defined by their approach) can represent an improvement over what the authors call

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"diagnostic" (analysis of historical data) and "prognostic" (analysis of model results) approaches. However, their method is a form of prognostic analysis, because they simply use projected climate (P and T) to drive calculation of a simple index of water availability (a highly simplified hydrologic model, seemingly). So this argument seems specious.

p. 2880, l. 15 "the HL classification to show how results from six potential future climate realizations may impact water resources." Vulnerability is mentioned but not defined.

p. 2882, l. 5. "accumulation of a seasonal snowpack in the Cascades, with annual average depths ranging from 7620 to 13 970mm (Ruffner, 1985)" Express in SWE, use Snotel?

p. 2883, l. 14 - Is S' (as implied) based on mean monthly data over the reference periods (i.e., mean monthly values for 1971-2000 and 2041-2070, or a total of 24 values)? Or is S' recomputed for each year of the simulation based on modeled values of P and T? Please clarify.

p. 2884, l. 23 to p. 2885, l. 13: why only two models? Why only use predicted GHG emissions as the criterion for model selection? Given the high variability among models (demonstrated by differences even in the two models shown here) there is great uncertainty about future hydrology. Why not pick models that differed from one another in terms of the drivers of hydrology?

p. 2885, l. 14-15: so 1/8 degree data were interpolated to 400-m data? Please clarify. How was the interpolation validated? Were any attempts made to relate the interpolated values to any measured data?

p. 2886, l. 19-20. S_i represents the area-weighted monthly watershed positive surplus from each 20 of the n assessment units in a basin, and A_i is the area of assessment unit i . three case study basins (Fig. 1): the Siletz and Sandy, in western Oregon, and the Middle Fork John Day in eastern Oregon.

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p. 2887, l. 3-8. Because groundwater turns out to be a big deal in mediating results (at least according to the discussion), the way the groundwater index is determined should be presented here, and the indices for the case study basins should be presented in the methods. "Wigington et al. (2013) use the Q/S_i ratio to assess whether the river experiences groundwater losses or gains: a $Q/S_i > 1$ indicates that runoff is greater than available surplus, and thus suggests groundwater imports (changes in storage are assumed to be zero since 30 year normals are used). Conversely, a $Q/S_i < 1$ suggests groundwater exports, since runoff is less than available surplus."

p. 2887, l. 10-13. First mention of the fact that each unit was assigned an initial class - maybe add a sentence to the introduction summarizing the distribution of classes in Oregon, and include a map showing how basins were classified. "The percentage of assessment units that change class ranges from 4.4% for the ECHAM_B1 realization to 18.3% for PCM_A1b, with a mean of 10% over all six realizations"

p. 2904, l. 5. "The specific effects of these changes in timing and delivery of S_i are mediated by the geology of the basin. We discuss in detail results from three case study basins to demonstrate how the HL approach can be useful for understanding climate change impacts in diverse hydroclimatic and geologic settings, and to illustrate how the approach could support management." Given that these factors aren't mentioned in the introduction and methods, this comes as a big surprise. The methods section does not contain any information about how parts 3, 4, and 5 ((3) aquifer permeability, (4) terrain, and (5) soil permeability) of the classification system are estimated, nor does the introduction anticipate how these factors might influence runoff response. If this is the big story, it should be mentioned in the introduction, the methods for these parts of the classification system should be presented, and the logic for selecting the case study basins should be clarified.

p. 2905, l. 8. "we have demonstrated that the Wigington et al. (2013) HL approach can provide a method for mapping and interpreting vulnerability to climate change." Many methods could be used to map and interpret climate change - but we would like

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to know whether this method is accurate. The discussion does not address this.

p. 2905, l. 24 and l. 27. "the relationship between modified surplus and runoff is not quantified," and "Use of a model that estimates Q from S_* would allow for more objective conclusions." Please elaborate. I don't think it's a matter of objectivity; it seems more a matter of accuracy. It looks like S_* deviates significantly from observed Q in the case studies. This suggests that S_* does not accurately predict runoff (Q).

p. 2916 Table 2. These changes are based on the change in precip or temperature between the reference time period (1971-2000) and the target future time period (2041-2070) in the GCMs? Please expand table caption to clarify.

p. 2917 Table 3. all climates get drier. Makes sense: precip does not change, temp increases. Except for scenario B1 in which some climate classes get wetter. Since scenario B1 has greenhouse gas forcing, it is hard to understand how climate would get wetter.

p. 2920. Table 4. All 7 summer-peaking water surplus units move to spring peaking; and 35% of spring peaking water surplus units move to winter peaking.

p. 2922. Table 5. HL class definitions should be provided in the caption; readers should not be expected to look these up in another publication.

p. 2924. Figure 2. Why are only a subset of basins in Oregon highlighted in these maps? If only some parts of Oregon have been classified by previous work, can those original basins and their classifications be shown? Overall, it appears that the GCM simulations do not adequately capture the very strong spatial gradient of climate, because the changes in climate attributable to model projections show a strong gradient.

p. 2925. Figure 3. Again, please clarify why Figure 2 covers only selected portions of Oregon while Figure 3 covers the whole area. P. 2930. Figure 4. Why does seasonality change only in the high-elevation portions of Oregon? Is this simply a reflection of the spatial distribution of snowpack, i.e., it is orographically enhanced?

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p. 2931. Figure 5. Part (a) shows the expected pattern from future climate change: increased water available for runoff in winter and declines in summer, due to declining snowpack/shifts to rain in winter, and the expectation that this will enhance summer drought. However, part (b) does not make sense: if absolute changes are negative for all models in the month of July, how can percent changes be positive? Something seems wrong here.

p. 2932. Figure 6. Map shows expected changes: biggest losses of water surplus in the mountains for May, June, and July, because models lead to expected losses of snowpack and associated water storage.

p. 2933. Figure 7. In part (b), it appears that S_* anticipates Q by about a month, and is lower than Q , except in Oct and Nov. This implies that the water budget used to estimate S_* overestimates actual ET.

p. 2934-5. Figures 8 and 9. In part (b), that S_* is quite different from Q - S_* is higher than Q in fall and spring and lower in winter. What accounts for this difference?

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