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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #1

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The paper presents a novel way of assessing the impact of climate change on watersheds using hydrologic indices (climate and seasonality). The authors describe these changes using bias-corrected and downscaled GCM data from two GCMs for the entire state of Oregon and then in detail for smaller regions and watersheds. Overall, I find the paper to be extremely well written (albeit somewhat long) with a coherent structure. Just by reading it you can tell it was went through multiple iterations of review by multiple people. The discussion and conclusions are based on sound results. Therefore, I feel that my comments are only somewhat minor in nature. Mostly, my comments deal with the authors better explaining certain items. See comments:

[1] In the abstract, I don't feel as if the authors do a good job at the beginning of explaining where they are doing this assessment. It takes a few sentences to mention Oregon.

[2] The authors should describe the land use/cover in Section 2.1. Land use/cover can have a large control over hydrology so I believe it's worth describing.

[3] Why did the authors use the Hamon method to estimate PET and not a more sophisticated method?

[4] I'm a bit confused where PACK comes from. Is this observed, modeled, estimated?

[5] The authors mention the sensitivity of the ECHAM and PCM models, stating that these models represented the highest and lowest global sensitivity. However, this is likely to not be true at the local scale. The authors should present the projections from these two models as percent changes in precipitation or increases in ave. monthly or annual temperature. This will give the readers an idea of just how extreme these GCMs are. It will also help out with the interpretation of the results. Authors can find this information at www.climatewizard.org

[6] Building on [5], most climate change impact studies use many GCMs to bracket the overall uncertainty and overall a more robust conclusion regarding the mean, max, and min impacts. Can the authors comment on why only two GCMs were chosen? I realize that these are supposed to be the max and min projections (see [5]), but using these two models doesn't give an overall idea of the mean projection.

[7] There are instances in the paper where the authors use the phrase "observed changes". These aren't actually observed datasets but projections, right?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2875, 2014.