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Discussions

Interactive comment on "Quantifying projected changes in runoff variability and flow regimes of the Fraser River Basin, British Columbia" *by* Siraj Ul Islam et al.

Anonymous Referee #2

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This is a generally well-written paper that discusses changes in runoff variability and flow regimes in the Frasier River Basin under climate change. The authors analyze 21 downscaled CMIP5 simulations that have been used as input to a VIC model implementation at 0.25° resolution. While the paper is generally well-written, the study itself is mostly routine and is not sufficiently novel in its current form that I can recommend publication in HESS.

General comments:

1. The paper is purely descriptive in its analysis. The authors describe the results from the model simulations, but make no real attempt to analyze and interpret them. For

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example, which specific processes contribute most to the increase in runoff variability? How does this increased variability affect the salmon population (the authors repeatedly make the point that the Frasier supports the largest migration of Pacific Ocean sockeye salmon in the world)?

2. As is, the findings mostly have regional interest, as there is no new methodological development nor are any of the findings particularly surprising. Warming in climates with a large seasonal snow component will lead to larger flows in winter because of a shift from snowfall to rain, mid-season melt, and earlier melt, but this has been widely reported for similar basins in western North America and Europe. The field has progressed to where this may be extremely useful information for local water managers, but the study design and findings by themselves are not sufficient for publication in a scientific journal.

3. The paper is not significantly different from an earlier paper by the same lead author in Journal of Hydrometeorology (doi:10.1175/JHM-D-16-0012.1), which discusses the same modeling chain and setup (some of the figures are near identical and should be attributed at the very least). That paper is based on a smaller model ensemble and focuses more on changes in the mean climate / hydrology rather than changes in variability. If the authors choose to focus on variability in this paper, then I would encourage them to analyze what this increase in variability actually means for the basin.

4. The manuscript lacks a clear conclusion section as the authors have a single combined discussion and conclusion without a clear take-home message. I would suggest splitting these components as it emphasizes the need to have a clear conclusion that adds to the existing body of knowledge and that is focused on findings that are of wider interest than the local changes in the Frasier River basin.

Specific comments:

a. p.5 l.3-4: While the authors state that it "[...] is important to evaluate such regime transitions on regional scales while characterizing snowmelt and rainfall driven flows independently", they never clearly state why this is important and how they will use this analysis.

b. Section 2: This section should reference their earlier work (Islam et al., 2017) more directly, as much of the model setup is the same, in particular the setup of the hydrological model. As is, the section is rather uneven. It goes into great detail regarding the resolution of the ANUSPLIN dataset ("having a spatial resolution of about 9.26 km in the meridional direction and one that varies proportionally to the cosine of latitude in the zonal direction.") but says nothing about the VIC calibration or setup. Incidentally - is the ANUSPLIN dataset simply a 5 arcmin resolution (1/12°)?

c. Section 2.2: In addition to the strengths, the authors should also address the shortcomings of the downscaling techniques that they use, especially since they look at variability in daily time series. For example, Gutmann et al. (2014) noted that BCCA overestimates wet day fraction and underestimates extreme events. Perhaps the combination with BCCI fixes this, but that would be good to discuss.

d. p.9 l.1: Wu et al. (2011) does not describe a routing scheme, but simply provides routing networks at different spatial resolutions. From the sentence that follows it appears that the authors have used the Lohmann routing scheme. This should be clarified.

e. Section 2.3: The authors do not provide sufficient detail about the VIC setup. It is fine to refer to their earlier paper, but it would be good to mention model resolution, a two-line summary of the source of the parameters, etc. That would be more useful than the long list of references to previous uses of the VIC model (p. 9 second paragraph).

f. p.11 l.1-2: "Peak runoff during the cold season was computed between 1 October and 1 March when the 3-day running mean daily air temperature exceeds 0°C at each gridcell." Why the extra condition based on air temperature?

g. Section 2.4.2: This section needs to be streamlined. The equations are unnecessary, since most of us know how to calculate a mean and variance for a data set.

h. In the results section I found the narrative hard to follow in part because of the way in which the authors use abbreviations to refer to the different sub-basins. Sentences such as "The advance in the timing of the annual peak flow in these sub-basins is slightly less than for the FRB as a whole (∼20 days for UF, ∼18 days for QU, ∼25 days for TN and ∼35 days for CH) [...]" are difficult to read. The numbers may be more effectively presented in a Table, which allows the text to focus on some particular insight that can be derived from this.

i. Figures were generally of good quality.

References:

Gutmann, E., T. Pruitt, M. P. Clark, L. Brekke, J. R. Arnold, D. A. Raff, and R. M. Rasmussen, 2014: An intercomparison of statistical downscaling methods used for water resource assessments in the United States. Water Resour Res, 50, 7167-7186, 10.1002/2014wr015559.

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