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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate change impacts on snow and streamflow drought regimes in four ecoregions of British Columbia" *by* Jennifer R. Dierauer et al.

Anonymous Referee #1

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General comments This manuscript aims to analyze the impacts of climate change on the evolution of snow and streamflow drought as well as their propagation relationship in four very small basins (<10 km2) in British Columbia. They used a combination of climate and hydrological models. However, I don't quite agree that there's enough innovation (similar to the mix of their two previous papers which have been published in WRR, such as Dierauer et al., 2018 and 2019) in this paper and even some methods may be problematic. Thus, the contribution of the current version of this manuscript is rather marginal and the study does not justify a novelty appropriately for HESS publication. Major concern: Although they describe the possible changes in drought propagation (from snow drought to streamflow drought), the full manuscript mainly focuses

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on the individual changes of snow and streamflow droughts (e.g., the frequency and severity, very conventional drought characteristics). Actually, the drought propagation has its own property, such as the propagation time. What happened to the propagation time from snow to streamflow droughts and what might change in the future for the study regions. How to build the relationship (linear or non-linear) between snow drought and streamflow drought and analyze its changes? Unfortunately, I did not see anything like this. In addition, the authors did not consider the drought persistence (e.g., duration) when defining the snow and streamflow droughts. There may be cross-seasonal droughts. As a result, at this stage I am suggesting to reject (or major revise) the manuscript. Other comments: (1) Paragraph 10: the definition of snow drought looks more like meteorological drought (e.g., below-normal precipitation). (2) Table caption: the catchment characteristics should be more detailed, such as "Catchment characteristics (e.g., name, location, area, and etc.), including. . .. . .". (3) The study basin area is too small, how accurate the downscaled is, especially for the daily climate data. Because the author uses the daily simulated streamflow series, the author needs to add the comparison results of daily downscaled and observed data (e.g., the precipitation and temperature). (4) Many previous studies have shown that the same climate models differ in streamflow simulations on different hydrological models. How the author considers it? The author should be adding more discussions. (5) Paragraph 25: hydrological drought » summer streamflow drought or streamflow drought? (6) The author should add the calibration and validation results of historical period (e.g., 1980s period). (7) 3.1.4 Section: Are the averages of climate model simulation results used and then analyzing drought results? Or each climate pattern with a drought result and then an average? Confused. (8) 3.2.1 Section: Confused. I don't know why this defines snow drought? For example, what is the below-normal peak snow water equivalent? Why use the term 'Years'? Since the differences in the catchment and climate features of four study basins are very obvious, why use the same T25<0âĎČ (or T25≥0âĎČ) to define the "winter . . ." (or "summer . . ."))? (9) Authors should pay attention to the difference between "streamfow" and "runoff". The full manuscript should be used in a

uniform manner. (10) 3.2.2 Section: Low flow is not equal to streamflow drought. How to consider the ‘branch drought’ in a long-lasting drought?

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