Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-676-RC2, 2020

Hydrology and Earth System Sciences
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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 #2

Received and published: 20 March 2020

Dierauer et al present an interesting study that aim to evaluate the impact of climate change on snow and streamflow drought in four small watersheds of British Columbia, partially based on the methodology presented in two previous studies published in WRR (2018 and 2019) by the same authors. To do so they set up a hydrological model MIKE SHE forced by statistically downscaled climate data under the RCP 4.5 and 8.5 scenarios and three GCMs. The mayor concerns I have about this paper, is how the model simulates the snowpack and the intrinsic assumptions and uncertainties that arise from this simplified approach (degree day). Realistically representing the snowpack is critical to the analysis the authors perform, as it is all based on the identification of snow droughts. I have serious doubt about the capacity of this model to properly

capture changes to the snow dynamic under the future scenario, or even if it can properly simulate historical snow (not shown). The authors argue that they only care about change to relative (historical vs projections) and not absolute changes (lines 13-15, page 9), then Figure 2 shows how not even relative changes are being well captured, not at least when compared to a more reliable energy balance. Note that this is not only about getting peak SWE right, but the timing is also not properly captures either (fig 2a vs 2b; differences between peak swe and depletion timing), as this is absolutely critical for groundwater recharge and streamflow, and the following overall analysis. I think the authors need to make a more compelling case, in which snow accumulation and melt (volumes and timing), and streamflow are being adequately represented by the model. Otherwise, any hydrological projections will lack scientific support. Therefore, following HESS's high publication standards, I recommend major revisions and resubmit when these problems are fixed. Below you will find more general comments that may help in your reviewed version of the manuscript: 1. A much-improved site description is needed. You emphasize groundwater however; no description of soils and geology is given. It is not clear why you chose these 4 small watersheds. How representative are these of each ecoregions, or they just happened to be in those ecoregions? If you are going to focus on the ecoregions more than the watersheds you need to describe them better, how do they differentiate and what's unique about them. Table1: values of climate are given yet there is no source for this information. Are there weather stations, stream gauge, snow pillow in these watersheds to compare the model with? 2. You emphasize that you are creating a "generic" model as an interpretative tool (line 23, page 5). I understand that, but even when doing so you need to show that the model can represent in a realistic way the hydrology. Even in a processes or physically based models, one can get very strange behaviors when using default values (as used in this study), you need to show this is not the case. 3. Many modeling decisions that need to be better supported, particularly in terms of parameters. 4. Can you include any streamflow and SWE observation to support your modeling decisions? 5. Figure 2: Include the other 2 watersheds. As is only the Blueberry site looks reasonable. Capilano is very different between DD and EB. 6. Your definition of “winter” (page 10) is not very intuitive or clear, when do you starts counting to define your percentiles, January 1? Can you show some example of what does this translate into?

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