**Response to Reviewer # 2 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.

***a. The FRB exhibits substantial spatial variation of air temperature and precipitation due to its complex topography and maritime influences. The hydrologic response therefore varies considerably across the basin differentiating its flows mainly into snow-dominant or hybrid (rain and snow) regimes. These distinct flow regimes are expected to change under future climate change with hybrid or rainfall-dominant flow regimes becoming more prevalent. Such changes will most probably accelerate earlier onset of spring snowmelt and will increase magnitude of summer flood events in snowmelt-dominant flow regimes and will increase winter flows and flood events in rainfall-dominant flow regimes. Therefore the quantification of flow regime transitioning is extremely important for the FRB that could have implications for reginal adaptation measures and water resources management in the region.***

***To quantify such changes, we have used the snowmelt pulse detection technique to characterize snowmelt and rainfall driven flows independently. This technique clearly filters snowmelt-dominant flows from rainfall-dominant flows using the maximum cumulative departure within the define time window (Fig. 3 in the manuscript). In our revised manuscript, we have included a paragraph describing importance of flow regime transitioning in section 4.3. We have also further clarified snowmelt pulse detection technique in section 2.5 of the revised manuscript.***

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_)?

***b. We have revised section 2 to make it more informative about the VIC model setup and calibration and removing unnecessary details about the ANUSPLIN data. Yes, the ANUSPLIN data are of 5 arcmin resolution.***

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.

***c. BCCAQ2 does, in fact, reduce the magnitude biases in BCCA, as pointed out in the Werner & Cannon (2016) reference given in our Sec. 2.2 (p. 8). More specifically, as reported in a recent paper by Li et al. (2018), "BCCAQ avoids these issues by separating the downscaling and bias correction operations: BCCA, which includes a quantile mapping step at the GCM scale and subsequently generates realistic fine-scale spatial variability, precedes the***

***application of second quantile mapping at each grid point to further correct quantile distributions at the fine scale. Furthermore, the quantile mapping algorithm that is used explicitly preserves the climate change signal additively for temperature and multiplicatively for precipitation of the underlying climate model projections (Cannon et al. 2015)." According to Werner & Cannon (2016), BCCAQ "really shone for use with modelling hydrologic extremes. In this context, it exceeded all other methods." Further, as modified via BCCAQ2, it is especially suited to climate change applications, as featured in Cannon et al. (2015). That said, any bias correction method is only as good as the target data set used, and in this respect, the known biases of ANUSPLIN (e.g., the low precipitation bias at high elevations) are of course transmitted to the downscaled model results via BCCAQ2. This is a point we have now made explicitly in Sec. 2.2 of the revised manuscript, and added additional references as necessary.***

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.

***d. Thanks for pointing this out. We have now revised these sentences and have clearly stated that the Lohmann et al. routing scheme was used to extract runoff at basin outlet.***

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).

***e. In our revised manuscript, we have now included a paragraph under section 2.3 describing the VIC model resolution, parameters and it calibration and validation for the FRB.***

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?

***f. Using the extra condition based on air temperature helps to identify the end of the cold season more precisely in each year. The last day of the cold season therefore depends on the temperature criterion. A 3-day running mean is used to avoid extreme events when daily mean temperature exceeds 0°C for a given day within the cold season. In our revised manuscript, we have clarified this point under section 2.4.1.***

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.

***g. In our revised manuscript, we have removed these equations and have explicitly defined all the symbols.***

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.

***h. As per the Reviewer's suggestion, we have deleted this parenthesized portion in the revised manuscript, and have included these numbers in the form of a table in the supplementary document.***

i. Figures were generally of good quality.

***i. Thank you.***

**References:**

***Cannon, A. J., Sobie, S. R. and Murdock, T. Q.: Bias correction of GCM precipitation by quantile mapping: How well do methods preserve changes in quantiles and extremes? J. Climate, 28, 6938-6959, 2015.***

***Li, G., Zhang, X., Cannon, A. J., Murdock, T., Sobie, S., Zwiers, F., Anderson, K. and Qian, B.: Indices of Canada's future climate for general and agricultural adaptation applications, Clim. Change, 148(1–2), 249–263, doi:10.1007/s10584-018-2199-x, 2018.***

***Werner, A. T. and Cannon, A. J.: Hydrologic extremes - an intercomparison of multiple gridded statistical downscaling methods. Hydrol. Earth Syst. Sci., 20, 1483–1508, 2016.***