Thank you for the opportunity to submit our response to referees' comments on our manuscript titled "Evaluating uncertainties in modelling the snow hydrology of the Fraser River Basin, British Columbia, Canada" by Siraj Ul Islam and Stephen J. Déry (Reference # HESS-2016-469). We are thankful to anonymous Referees #1 and #2 for their comprehensive and constructive comments on our manuscript. We fully recognize and appreciate the referees' reports on our research. Indeed, incorporating their insights will lead us to improve our manuscript through the revision process. We are thus taking full consideration of the referees' comments and are preparing detailed responses listing a point-by-point response to each comment/suggestion along with the information on how the paper is being revised as per the two anonymous referees' suggestions. A complete and detailed response document will be submitted once a decision has been made on our discussion paper. Here we provide a general overview of our responses (in bold italic) to the major comments submitted by each referee. All minor comments will be addressed in our detailed response document.

Please let us know if additional information or points of clarification are required.

**Referee # 1:**
This manuscript provides an assessment of the uncertainties in hydrological modelling over the FRB, BC, Canada. The authors used four precipitation dataset and a calibration routine to quantify and characterize the uncertainties to related to meteorological and parameters estimates. The manuscript is well written and covers an interesting topic, however, I feel some issues have not been properly addressed. I suggest moderate revisions to the manuscript before publication. In general I think some of the procedures could be implemented more consistent to ensure that the conclusions of the paper are better.

***We recognize that there are complexities in our procedures that were not thoroughly addressed. In our revision process, we are paying special attention to ensure our results and discussion is more consistent by means of additional hydrological model simulations and analyses.***

Page 11 Line 19-20 The authors want to study the impact of different forcing dataset and their related uncertainties in hydrological simulations. They mention a couple of times that the errors in mountainous precipitation might cause significant discrepancies between dataset and therefore they use four different datasets to study that impact. Although the datasets all have their own resolution, the authors decide to bilinear interpolate all of them to a common grid of 25km without taking into account elevation corrections. I believe this procedure will add to the forcing uncertainty, especially for the coarser resolution since they are downscaled to a resolution without including the high resolution elevation data to correct for orographic effects. I think it would be good if the authors could provide some estimate for the uncertainty added by the bilinear interpolation without conditioning on the elevation profile.

***We agree that the elevation correction should be explicitly accounted for in the interpolation methodology to correct for orographic effects. Such correction is highly important when interpolating from coarser to higher spatial resolutions, especially in mountainous terrain (Dodson and Marks, 1997). However, in our interpolation methodology the NARR (32 km) dataset is interpolated from coarse resolution curvilinear grids to slightly higher (25 km) resolution rectilinear grids. On the other hand, both PCIC (6 km) and ANSUPLIN (10 km)***

***datasets are interpolated to a coarser resolution (25 km). Interpolating NARR datasets from 32 km to 25 km resolution does not induce much elevation dependent uncertainties since the change in orography remains minimal between mean elevations at 25 km and 32 km grid resolutions (Figure A). Thus the relationship of atmospheric variables such as air temperature with elevation remains nearly identical at both resolutions. In the revision, we will nonetheless highlight this source of uncertainty in our methodology section and will add details about the bilinear technique used in the interpolation process.***

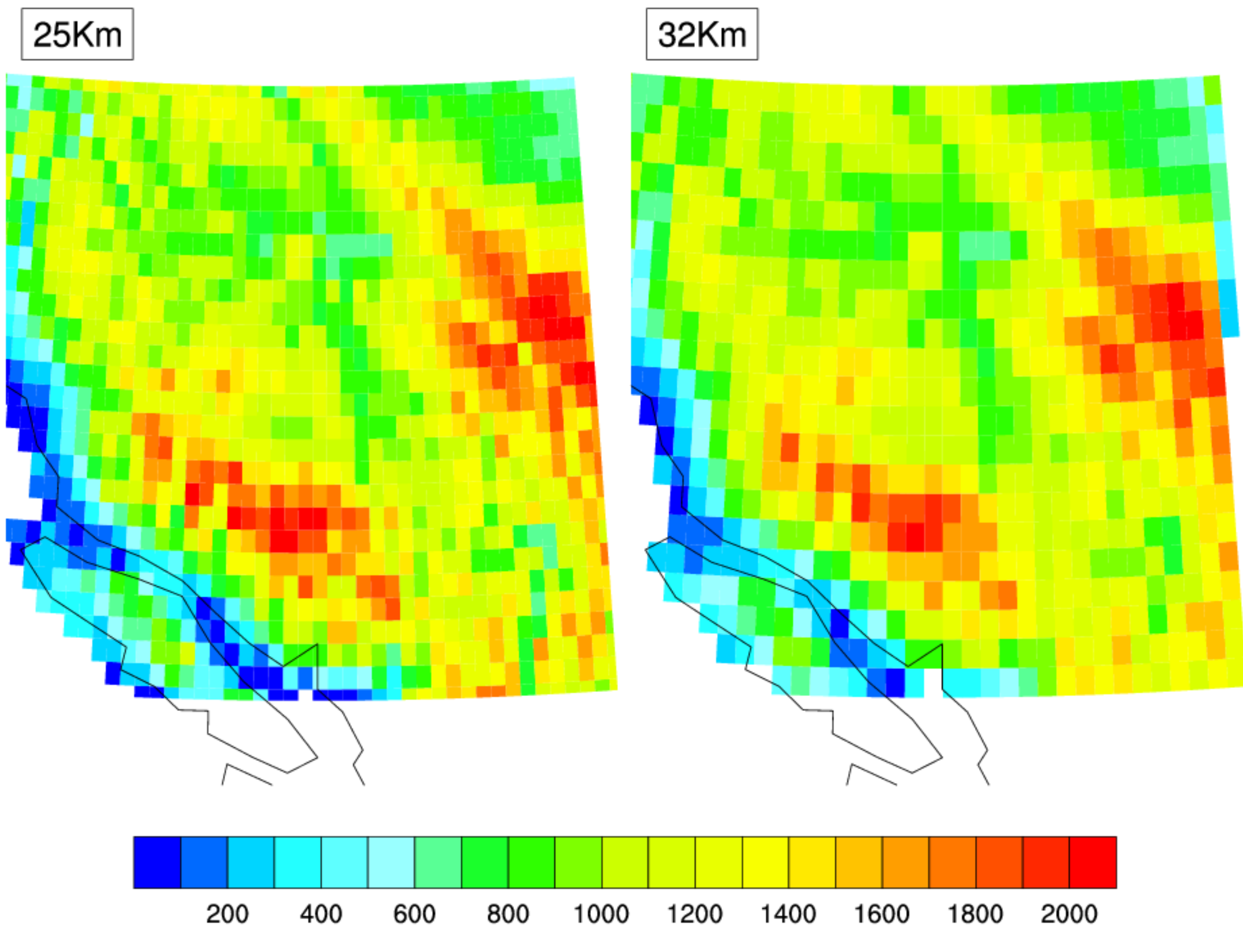


***Figure A: Comparison of mean elevation (m) at 25 km and 32 km resolutions over the part of British Columbia covering Fraser River Basin.***

Section 2.4.1 Why did the authors select these parameters? Is there sensitivity information that could be used to identify the most sensitive parameters? Maybe the impact of the routing model is more prominent, while it is not calibrated and doesn't account for the reservoirs and lakes present in the FRB. In addition, none of the snow parameters is calibrated, while the authors mention the importance of snow throughout the manuscript. Maybe a calibration on the snow processes (compaction, sublimation or just simple degree day factor), would further benefit the discharge simulation at the outlet and for the sub-catchments.

***We selected these parameters for calibration based on the manual calibration experience from previous studies such as by Nijssen et al. (1997), Su et al. (2005), Shi et al. (2008), Kang et al. (2015, 2016) and Islam et al. (2016). VIC is a physically based hydrologic model that has many (about 20, depending on how the term "parameter" is defined) parameters that must be specified. However, the usual implementation approach involves the calibration of only six soil parameters. Such parameters have the largest effects on the hydrograph shape and are the most sensitive parameters in the water balance components (Nijssen et al. 1997; Su et al. 2005). These parameters must be estimated from observations, via a trial and error procedure that leads to an acceptable match of simulated discharge with observations.***

***For the snow calibration, we have fixed the value of thresholds for maximum (at which snow can fall) and minimum (at which rain can fall) air temperature as 0.5°C and -0.5°C, respectively. These values were adjusted based on the region's climatology and are kept constant for all the simulations in the global control file. Parameters related to the snow albedo are adjusted using the traditional VIC algorithm based on the US Army Corps of Engineers empirical snow albedo decay curves for transitions from snow accumulation to ablation. We will be adding this detail in Section 2.4.1 in our revision process.***

Page 15, why is on the PCIC forcing data used for the optimizer. This almost ensures that the PCIC will have the best performance in the following evaluation sections. It might be more interesting (and more work), to calibrate for every forcing dataset individual and then use these four parameter sets for the validation with every unique forcing dataset leading to sixteen combinations. This also gives you four simulations per forcing dataset as a result of the different calibrations. I know this is some work, but it is feasible. I feel it would lift the quality of the overall uncertainty analysis and thereby better support the conclusions of the paper.

***As discussed in our manuscript, the VIC model is quite sensitive to the meteorological forcing data, most notably precipitation. This means that if the forcing data change, the soil parameters that result in reproducing simulated streamflow will change accordingly. Our comparison of hydrological simulations driven by four forcing datasets revealed that PCIC simulations are noticeably better with high NSE values and reproduce hydrographs similar to that from observations. Therefore, we only used PCIC forcing data for VIC model simulations to investigate the uncertainties in the model calibration process. Our primary goal is to evaluate optimizer sensitivity to a unique set of parameter limits. We want to see how the MOCOM optimizer results in different optimized parameters and change the overall simulated hydrograph in the calibration process. Evaluating the same sensitivity for each dataset is beyond the scope of this paper. However, in addition to the PCIC dataset, we have now repeated our methodology with the UW dataset. As expected, the optimized final values of calibration parameters are different for each set of initial parameter range producing three different hydrographs at Fraser River, Hope. We will be adding this detail in our revised manuscript. Thank you again for your thoughtful review and for highlighting these difficult but important questions.***

***Along with the above discussion, all the minor comments will be addressed and incorporated in our revised manuscript.***

**Referee # 2:**

The objectives of this study, which focuses on the modelling of snow hydrology of the Fraser River Basin (FRB) of British Columbia (BC), Canada, using the Variable Infiltration Capacity (VIC) model forced with several high-resolution gridded climate datasets (i.e., ANUSPLIN, NARR, UW, and PCIC), were to comprehensively assess uncertainties related to: (i) driving datasets, (ii) optimization of model parameters, and (iii) model calibration during cool and warm phases of the Pacific Decadal Oscillation (PDO).

This is a very well-written paper that deals with relevant subject matters to the hydrological modelling community. The paper delivers sound results with respect to objective (i), however, I believe those associated with objectives (ii) and (iii) are somewhat incomplete. All the results are discussed with respect to mean values. I believe the strength of the paper would be increased if the interannual variability was discussed as well. Moreover, at the end of the paper, the reader is left with an incomplete take-home message; which begs the following question: how could we use the outcomes of the paper if we were asked to run the model to answer questions related to hydroelectric development or other water resources management issue in the FRB? Something is missing here, like a solid recommendation. Here are a few suggestions I can take from the paper: whenever hydrological modelling is performed one should: (i) analyse the stream flow record with respect to previously identified teleconnection correlations? (ii) perform various calibrations in order to find the optimal set of parameter values? (iii) have several snow elevation bands, may be every 100 m? The readers want a clear take-home message from the authors on how to use their findings. Incidentally, the authors made such a reference in the introduction of the paper, but did follow through in their conclusion (last sentence on p. 4, lines 18-20).

I have made suggestions in the following list of specific comments below on how to fulfill what I perceive as shortcomings. As a side note, I found a bit difficult the exercise of jumping back and forth between the content of the main manuscript and that of the Supplemental File. Perhaps, the introduction of the content of the Supplement File and the content of Section 3.1 could be transformed into a useful technical note.

That being said, I strongly encourage the authors to address these comments as I feel the paper could certainly be a significant contribution to the community.

***We agree with the referee's concerns for objectives (ii) and (iii). We are therefore revising our results and discussion with the set of additional VIC simulations. This is to improve the discussion and the overall take home message of our findings. To address and compare interannual variability of VIC simulations driven by four different datasets, we will be analyzing the simulated runoff coefficient of variation (CV) at the Fraser River, Hope for each simulation. We will also expand the conclusion section of our manuscript with recommendations for applications of hydrological modelling and uncertainties in water resource management.***

***Thank you for providing a list of useful studies related to our work. In our revised manuscript, we will include and relate these studies with our results.***

P. 14, lines 4-6, it is written: « The VIC model calibration is applied to the Fraser River's main stem at Hope, BC and the FRB's major sub-basins, namely the Upper Fraser at Shelley (UF), Stuart (SU), 5 Nautley (NA), Quesnel (QU), Chilko (CH) and Thompson-Nicola (TN) Rivers (Fig. 1a and Supplementary Table 1). »

- Does this mean there is a set of parameter values different for each sub-basin and one for the drainage area between Hope and the outlet of each sub-basin? It is common practice to do so with many distributed models, but I do not see anywhere in the paper the performance of the model with respect to each subbasin outlet. Have I missed something?

***In our manuscript, we focused on the Fraser River main stem at Hope, BC for analysis. However, we have calibrated the VIC model for all the major sub-basins of the FRB using each different forcing datasets and results are already presented in Figure 7 of the manuscript. In our revised version, we will add a new table reporting NSE performance of the model for all the sub-basins.***

P. 14, lines 16-18: to be consistent, should not the five-year spin up period been applied to all gridded datasets; that is from 1979 to 1985; or loop over at least a couple of years until convergence is achieved and then undertake the calibration exercise?

***We agree that the VIC model spin up period should be the same for all four forcing datasets. Except for the NARR driven VIC simulations, the PCIC, UW and ANUSPLIN driven simulations are integrated using a five-year spin up period prior to 1979. In our revised manuscript, we will loop recursively the NARR driven simulation for five years using the year 1979 as the forcing data and will re-calibrate the model to ensure our methodology is consistent with the other three sets of simulations.***

P. 15, lines 4-11: Why using the PCIC gridded dataset for the optimizer uncertainty runs? Why modifying the parameter space? Why not conducting several optimizer uncertainty runs and look at the various local optima? That is what is usually done. By reducing the parameter space, the authors are restricting, in theory, the number of local optima, yet there could still be several of them in this modified space. At the end we get three sets of local optima, yet there are multiple other sets. This is an interesting twist, but:

- Please provide more details on this calibration strategy. The outcome of this work is incomplete as I believe there should have been multiple calibrations for each experiment in order to fully illustrate the equifinality. Please discuss.

***The comparison of hydrological simulations driven by four forcing datasets revealed that PCIC simulations are noticeably better with high NSE values and reproduce hydrographs similar to that from observations. Therefore, we only used PCIC forcing data for VIC model simulations to investigate objective (ii). We agree that in the literature (e.g. Lindenschmidt et al. 2007; Shen et al. 2008; Sudheer et al. 2011, etc.), different methodologies and several optimizer uncertainty runs are used to address overall uncertainty in calibration parameters without modifying the parameter space. However our primary goal is to evaluate optimizer sensitivity to a unique set of parameters limits. We want to see how the MOCOM optimizer***

***results in different optimized parameters and changes the overall simulated hydrograph during the calibration process. One can see from our analysis that within a broad range of parameter limits, only one particular subset of parameter range is optimally converging and produces more reliable calibration results with high NSE values. The overall conclusion of this analysis is that the automated optimizers used to converge calibration parameters still rely on the hydrologist's experience and some manual adjustment of initial calibration parameter ranges. We will be adding this discussion in our revised manuscript. Furthermore, to facilitate concerns from Referee 1, we have now repeated this methodology with the UW driven VIC simulation in addition to PCIC-VIC simulations. We will be updating Table 3 accordingly in our revised manuscript.***

P. 15, lines 12-21: why not six (6) experiments to fully explore the question (2 cool phases x 3 warm phases)?

- This is an interesting experiment, yet I believe it is incomplete as not all possibilities were explored (see Seiller *et al*., 2012).
- Are not there any other highly-impacting teleconnection that affect the basin hydrology (*e.g*., ENSO)?

***We agree with the referee's suggestion. We have now extended our experiments by adding two additional PDO runs (Table A). As per the availability of forcing data, we can only evaluate PDO phases within the 1950-2006 time period. For each calibration experiment in one particular phase of the PDO, the automated MOCOM optimizer is used to optimize calibration parameters. The NSE is calculated for the calibration and validation periods using the daily observed streamflow data for the Fraser River at Hope. While evaluating additional PDO experiments, our conclusion remains the same, i.e. the calibration is biased toward the cold or warm phase of the PDO and therefore it is necessary to avoid PDO phase shifts in the hydrological model calibration and validation process. In essence, it is better to run multiple calibration experiments within the required time period.***

***Table A: PDO runs and their performance metrics (NSE coefficient) in the calibration and validation time period evaluated for Fraser River at Hope, BC.***

| | ***Calibration*** | | ***Validation*** | |
|---|---|---|---|---|
| | ***NSE (Time slice)*** | ***PDO Phase (Flows)*** | ***NSE (Time slice)*** | ***PDO Phase (Flows)*** |
| ***PDO1*** | ***0.84 (1981-1990)*** | ***Warm (low flows)*** | ***0.84 (1991-2001)*** | ***Warm (low flows)*** |
| ***PDO2*** | ***0.84 (1956-1965)*** | ***Cool (high flows)*** | ***0.85 (1966-1976)*** | ***Cool (high flows)*** |
| ***PDO3*** | ***0.84 (1967-1976)*** | ***Cool (high flows)*** | ***0.79 (1977-1987)*** | ***Warm (low flows)*** |
| ***PDO4*** | ***0.81 (1977-1987)*** | ***Warm (low flows)*** | ***0.86 (1967-1976)*** | ***Cool (high flows)*** |
| ***PDO5*** | ***0.84*** | ***Warm*** | ***0.82*** | ***Warm*** |

| | | | | |
|---|---|---|---|---|
| | *(1991-2001)* | *(low flows)* | *(1981-1990)* | *(low flows)* |

***The FRB is strongly teleconnected to PDO and ENSO phases. Our VIC implementation has been tested for its response to warm and cool phases of the PDO and ENSO. The model realistically simulates anomalies of runoff, SWE, and snowcover under these climatic conditions (Thorne and Woo, 2011). In Figure B (Figure 7 of supplementary document), the effects of the PDO on annual time series of air temperature, precipitation, $SWE_{melt}$ and runoff is shown.***

***Figures C and D represent the response of VIC simulated snowcover (SC) composite anomalies to warm (Figure C) and cool (Figure D) phases of ENSO. ENSO phases are identified in the observation to estimate composites. Consistent with the findings reported in other studies such as Mantua et al. (1997), Fleming et al. (2007) and Shrestha et al. (2016), the anomalies are below (above) normal in warm (cool) ENSO phases.***

***In our uncertainty analysis, we only focused our attention on the PDO teleconnections as the hydrological model calibration is usually performed over many years (more than 5 to 10 years) rather than a few individual years. As ENSO teleconnections are short term (a season or two), it is not feasible to evaluate its influence on the total calibration period.***

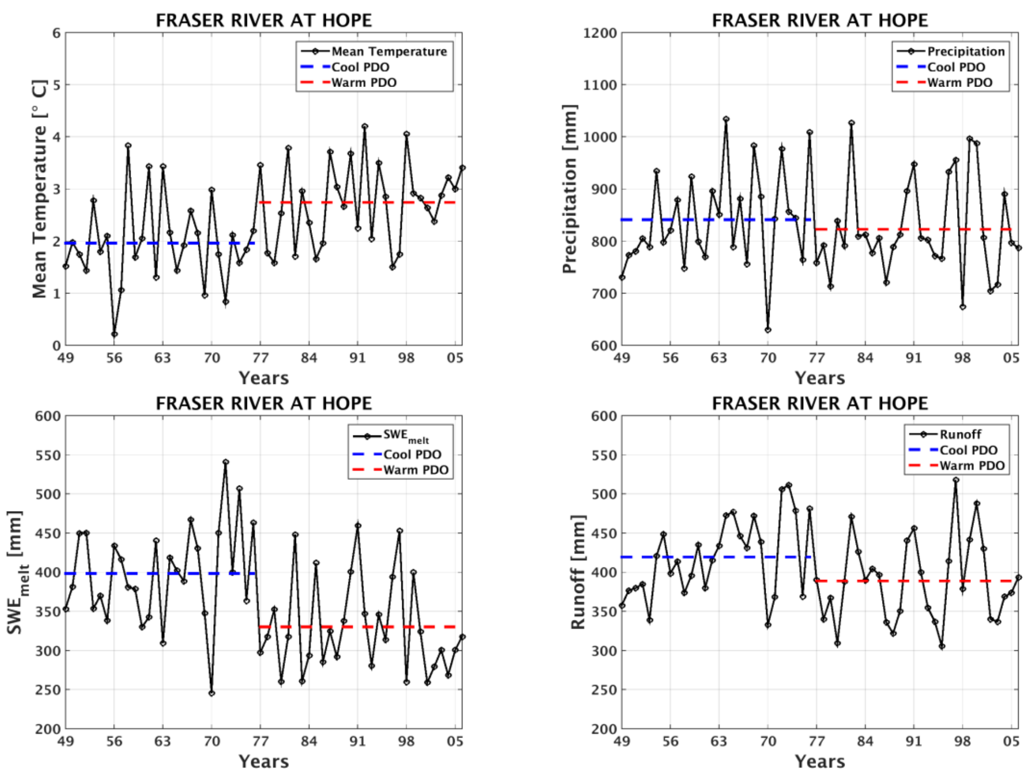

***Figure B: Annual variation of air temperature, precipitation, $SWE_{melt}$ and runoff in cool (blue line) and warm (red line) phases of the PDO. Variables are areally-averaged over all of the FRB's grid points.***

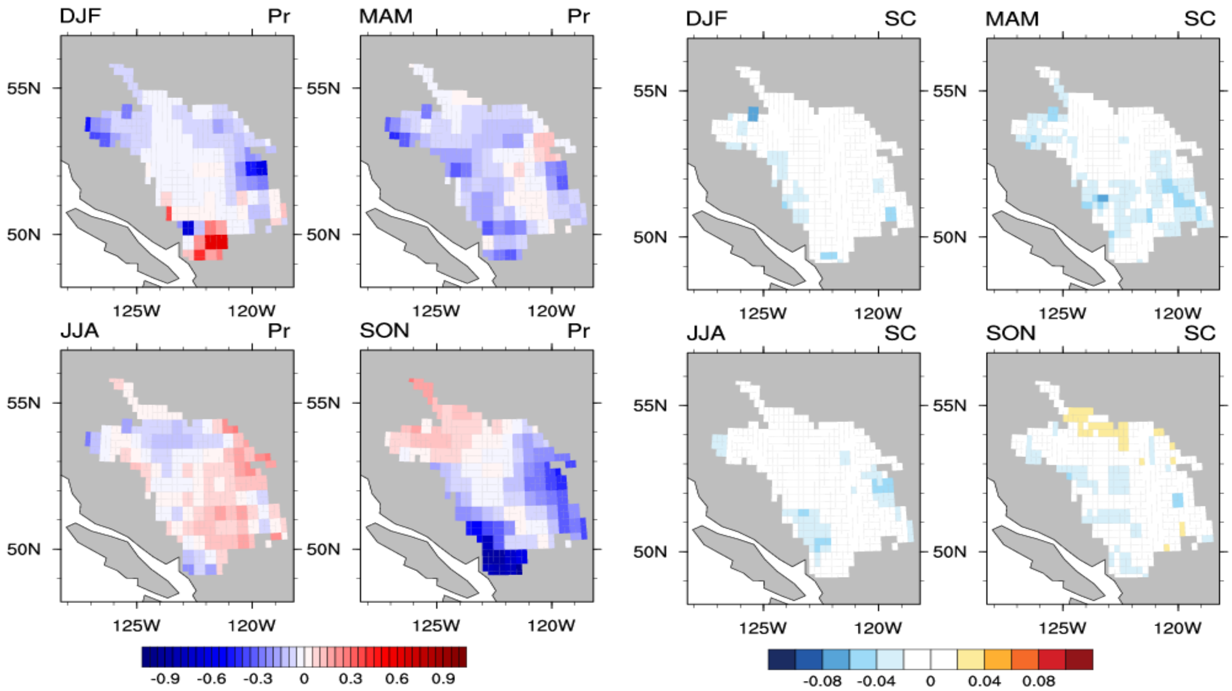


***Figure C: Seasonal composite of precipitation (Pr ~ mm/day) and snowcover (SC ~fraction) anomalies in El Niño years.***

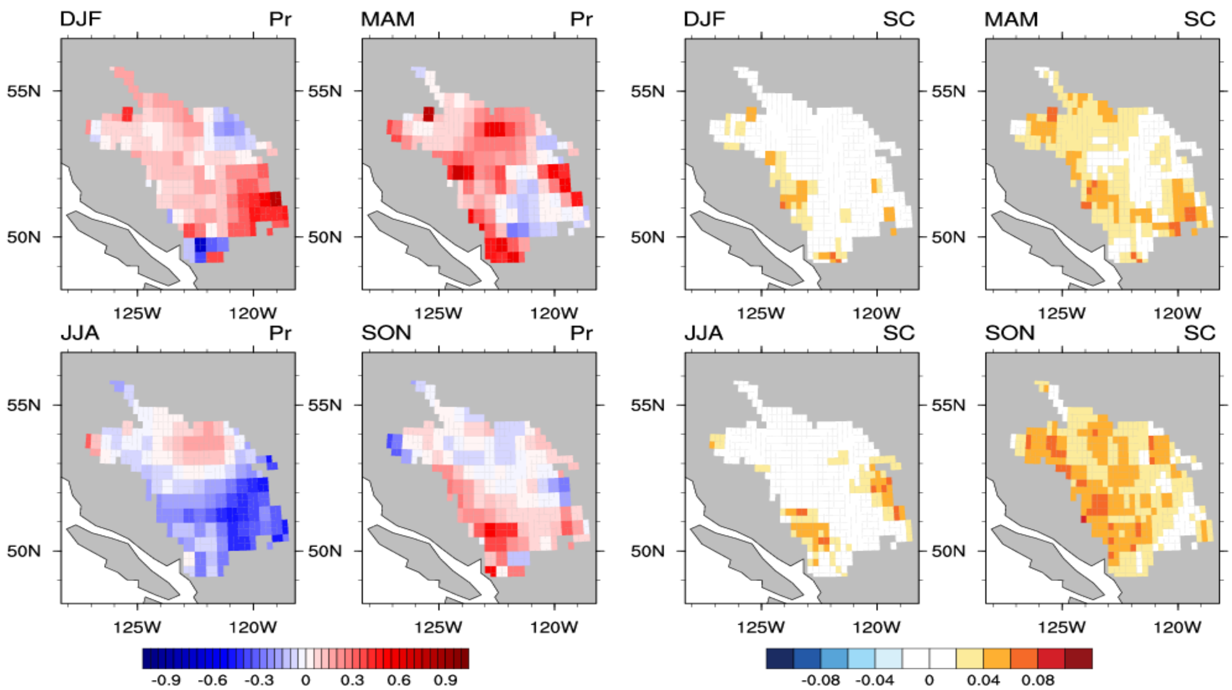


***Figure D: Seasonal composite of precipitation (Pr ~ mm/day) and snowcover (SC ~ fraction) anomalies in La Niña years.***

Given the results introduced in Section 3.1, I think it would have been interesting to have the following calibration parameters: snow-to-rainfall temperature threshold and a vertical temperature gradient.

- Does the VIC model have such parameters? If that is the case, why not calibrating them?
- Is there a publication that has introduced a formal sensitivity analysis of the VIC model?
- Why focusing on soil-related parameters solely and not others as well? In other words, the authors should first introduce to the readers all the model parameters and provide a rationale for their choice.

***Our response to referee 1 is copied here.***

***"We selected these calibration parameters based on the manual calibration experience from previous publications such as by Nijssen et al. (1997), Su et al. (2005), Shi et al. (2008), Kang et al. (2015, 2016) and Islam et al. (2016). The VIC is a physically based hydrologic model and has many (about 20, depending on how the term "parameter" is defined) parameters that must be specified. However, the usual implementation approach involves the calibration of only six soil parameters. Such parameters have the largest effects on the hydrograph shape and are the most sensitive parameters in the water balance components (Nijssen et al. 1997; Su et al. 2005). These parameters must be estimated from observations, via a trial and error procedure that leads to an acceptable match of simulated discharge with observations.***

***For the snow calibration, we have fixed the value of thresholds for maximum (at which snow can fall) and minimum (at which rain can fall) air temperature as 0.5°C and -0.5°C, respectively. These values were adjusted based on the region's climatology and are kept constant for all the simulations in the global control file. Parameters related to the snow albedo are adjusted using the traditional VIC algorithm based on the US Army Corps of Engineers empirical snow albedo decay curves for transitions from snow accumulation to ablation. We will be adding this detail in Section 2.4.1 in our revision process."***

P. 19, lines 12-15: « The lower SWE in the NARR-VIC simulation is probably due to the warmer air temperature during winter and spring (Fig. 2b). Winter temperatures being warmer in the NARR dataset may alter the phase of precipitation partitioning with more rainfall than snowfall, and hence less SWE in the NARR-VIC simulation. »

- This is formulated as a hypothesis, but do not the authors have the input precipitation dataset to validate this hypothesis?

***We have validated this result by a VIC model sensitivity experiment where the air temperature was perturbed by 2°C while keeping the precipitation unchanged. The simulated SWE and runoff decreased (nearly 25% decrease in peak runoff) with 2°C rises in air temperature forcings. The detail of this experiment is already mentioned in the manuscript (in its current version) at Page 22, Lines 15-19. We did not alter the phase of precipitation in this experiment considering that the VIC model automatically partitions the precipitation type (solid versus liquid) during its simulation.***

P.20, line 4: what about a sensitivity analysis of the impact of the number of elevation bands on the output variables of interest? Why ten in other words, why not twenty?

***The current literature focusing the VIC model mostly rely on 1 to 5 elevation bands in model implementations (Haddeland at al, 2002; Sherestha et al. 2012, 2016; Oubeidillah et al. 2014 and many more). Depending on the required research question and the available resources, a suitable number of elevation bands can be generated in the VIC model. In our implementation of the VIC model, we have used 10 elevation bands by utilizing a high resolution digital elevation model. Considering the complex terrain, computational time and 25 km resolution grid, 10 elevation bands are sufficient to investigate elevation dependent changes. Increasing elevation bands to 20 will cost more computation time without contributing much to the elevation dependent changes.***

P.26, lines 17-19: « For each set of calibration experiments, the calibration parameters are different, which affects the air temperature lapse rates and thus evapotranspiration, the formation of the snowpack, and the timing of snowmelt. »

- I thought the temperature lapse rate was not a calibration parameter or is it?

***The temperature lapse rate is not a calibration parameter. In the VIC model, mean grid temperature is lapsed to each elevation band. Precipitation falls as snow or rain depending on the lapsed temperature. The model defines the lapse rate based on the elevation and number of elevations bands during its simulation. We will modify this sentence in our revised version.***

P.28, lines 5-6: « The trend analysis further highlighted uncertainties of the NARR Dataset. »

- Uncertainties in terms of what...I am not sure I follow well the authors here. Please provide additional details.

P.29, line 12: not being familiar with the FRB, I was a bit surprise to learn here that the authors had never mentioned in the description of the basin that there are glaciers in the study area!

***In Alberta and British Columbia, glaciers account for an estimated area of 26,700 km$^2$ (Bolch et al. 2010). There are more than 1000 glaciers in the northern and central Rockies spanning across a total area of 838 km$^2$. However these glaciers contribute relatively little to the annual streamflow to rivers of western Canada. For example, Comeau et al. (2009) studied the glacier contributions to streamflow generation by applying a hydrological model at 9 km resolution to the North and South Saskatchewan Rivers originating in the Canadian Rocky Mountains. They separate the mean annual streamflow contributions from glacier retreat and summer melting of the seasonal snowpack. For the period 1975-1998, they estimate that glacier melt (ice volume losses) made up only 2.0% of mean annual discharge of the Bow River in Calgary.***

***In our study, the dynamics of glaciers cannot be simulated in the current version of the VIC model as glaciers and their dynamics are not included in the model physics. Nonetheless the effects of glaciers may not change our results significantly as we have used the VIC model on ~25 km grid cell resolution (625 km$^2$ area per grid cell) in our study. Furthermore, glaciers cover only 1.5% of the FRB (Shrestha et al. 2012) and provide only a modest contribution to streamflow, primarily in late summer (August/early September). Although glacier dynamics are not simulated in the VIC model there are some cells where there is a perennial snowpack. Year after year this water does not melt out. This is a recognized challenge with the VIC model and is dealt with differently in each implementation, ranging from eliminating these cells from analysis (as in our study) to introducing a simple, conceptual representation of glacier mass balance into VIC, modelled using perennial snow in combination with VIC built in snow routines where a portion of VIC grid cells are identified as glacier cells and used to form a glacier mask (Schnorbus et al. 2011). In this study, we compared those cells with perennial snowpack to Baseline Thematic Mapping (BTM) and found that the glaciating cells match the location of observed glaciers. However there were very few grid cells with a perennial snowpack due to the low resolution and were therefore masked in the analysis. We will explicitly mention this in our revised manuscript.***

***All minor comments will be addressed and incorporated in our revised manuscript.***

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