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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Evaluating uncertainties in modelling the snow hydrology of the Fraser River Basin, British Columbia, Canada" *by* Siraj Ul Islam and Stephen J. Déry

Anonymous Referee #1

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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

Major comments 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.

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.

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.

Minor comments I have discussed the ANUSPLIN acronym with some colleagues over the lunch break. We believe the authors could maybe come up with a better name.

Page 3 Line 13-14: measurement of the response metric -> objective function and the calibration variable

Page 7 Line4: Why did the author select the PDO rather than the more influential ENSO signal for Western Canada

Page 11 Line 8The PGF is not really high temporal resolution, it uses satellite observations, which are corrected with gauges at monthly temporal resolution. I think it would be good to provide the reader with the data sources of the PGF. This is important to understand the performance of the WU dataset

Section 2.2 and 2.4, maybe combine these sections since they both cover VIC and could be easily combined into on VIC section. Otherwise move section 2.3 forward to have the two VIC sections following one another.

Page 12 Line 19, maybe remove the number of columns and rows, the domain would be sufficient

Page 13 Line 3 Citation, year could be without the brackets

Page 14 Line 18 Why not loop over the year 1979 rather than no spin-up. If the forcing of 1979 were to be recycled for five year and the stabilized ICs could then be used rather than no spin-up. This would ensure that the NARR simulation is more equal to the others and therefore the difference can be really attributed to the difference in forcing rather than a cold model start.

Page 14 Line 18-20 Once calibration. . ...(Table 1) -> please clarify. It is not entirely clear what you want to do here.

Page 18 Line 9-12 Do you have estimates of the cross-correlation between the precipitation products. Up to what extend are they derived from the same input data.

Page 22 Line 15 mentions a VIC sensitivity experiment, it would be great to show some of these results to get a better understanding of the model parameter uncertainty

Page 24 Line 13-14 “air temperatures are more crucial for hydrological simulations”, I would argue that this is true for the timing, but not so much for the total streamflow volume (at least not for the FRB, where evap is low). Maybe rephrase to “are crucial for the runoff timing in hydrological simulations”

Figure 2 When does the water year start? October 1st? Please clarify the captions of the figures, they could be more self-explaining

Figure 7, What is a,b,c,d,e,f? No explanation, also not in the caption of Figure 6

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