

Interactive comment on “Diagnosis of future changes in hydrology for a Canadian Rocky Mountain headwater basin” by Xing Fang and John W. Pomeroy

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Response to referee #1's comments:

General comments The manuscript presents a climate change study for a small Canadian Rocky Mountain headwater basin. As process understanding has to be developed at the local scale, such regional/local studies are of a high value. The study is presented in a concise way and contributes well to current discussions. Small changes could improve the readability of the paper (see detailed comments). However, I would strongly recommend to add a discussion on the uncertainty of the hydrological modelling results. The results chapter is full of numbers, partly with a high number of

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positions after the decimal point suggesting a high accuracy. Depending on the model design and the catchment characteristics, some results of hydrological modelling are more reliable than others. If, e.g., a model does not represent hydrophobic effects and if they play an important role in a catchment, then the model may simulate overall runoff with a satisfying efficiency but the calculated portion of Hortonian overland flow calculated by the model will be less reliable in this case. Thus, for readers not familiar with CRHM, a discussion on the strengths and especially weaknesses of the model concepts and the resulting reliability of the model results would be very valuable.

Response to general comments: Thanks to referee #1 for general comments about the manuscript. We added a discussion on the strengths and weakness of model in Discussion section. We also revised the manuscript to improve its structure, readability and flow.

The comments in detail:

Detailed comment 1: The paper contains numerous abbreviations (WRF, MCRB, CRHM, CTRL, PGW, QDM, WY, . . .). A list of abbreviations would improve the readability. Response 1: Yes, we added an appendix to include a list of abbreviations to improve the readability.

Detailed comment 2: In the entire paper: please do not use “alpine” and “treeline” as single words, but always in combination with ecozone: “alpine ecozone”, “treeline ecozone”. This would improve the grammatical correctness of the sentences and the readability. Response 2: Yes, we added ecozone after “alpine” and “treeline” in the revised manuscript.

Detailed comment 3: I am no native speaker, but to my feeling sometimes articles are missing, e.g. Page 2, line 18: of the world, Page 2, line 23: two of the most Page 3, line 29: in the eastern slopes Page 5, line 5/6: the complex mountain terrain Page 9, line 23: had a very comparable . . . value (or had . . . values) Page 11, line 28: the entire basin Page 13, line 16: by a combination Page 14, line 15: a large elevational gradient

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Page 14, line 18: these changes were result of the interaction Response 3: Yes, we added these missing articles in the revised manuscript.

Detailed comment 4: Page 3, line 19 and page 4, line 20: A model does not permit convective precipitation processes (they are permitted by the atmospheric conditions), but it permits the representation/simulation/consideration of convective precipitation processes. Please adapt the formulation. Response 4: Yes, we adapted the formulation suggested by the referee.

Detailed comment 5: Page 3, line 20/21: I would delete “to combine . . . from CRHM to” as this is also said by “using a dynamically. . . model” (line 23-24). Response 5: Yes, we removed the redundant words and rewrote the objectives as: “The objectives of this paper are to use CRHM driven by WRF to: (1) evaluate the ability to simulate snowpack and streamflow regimes in a Canadian Rockies headwater basin without calibration; (2) diagnose the detailed changes in hydrology due to impending climate change for this headwater basin. By relying on physically based, uncalibrated simulations and dynamical downscaling, it is hoped that the approach introduces a highly robust method for evaluating the impacts of climate change on mountain hydrology.”

Detailed comment 6: Page 5, line 29: I would include a citation (e.g. Pomeroy et al., 2007) at the first appearance of the model. Response 6: Yes, we added Pomeroy et al., 2007 at the first appearance of the model.

Detailed comment 7: Page 6, line 1: Please explain dynamic networks of HRUs (in the models I have worked with, HRUs are defined for a catchment and remain the same during the whole simulation). Response 7: Yes, HRUs in this model also remain the same throughout the whole simulation, so we deleted “dynamic networks of” to be clearer and reduce confusion.

Detailed comment 8: Page 6, line 12: please give some short information on the June 2013 flood. Response 8: Yes, we added some brief information for the June 2013 flood: “. . .the updated model was evaluated in the June 2013 flood when approximately 250

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mm precipitation fell at MCRB during 17-24 June 2013 (Fang and Pomeroy, 2016; Pomeroy et al., 2016).”.

Detailed comment 9: Page 6, line 26 to 28: Most of the content of this sentence is repeated in the next sentence, please streamline this text. Response 9: Yes, we combined two sentences to improve the readability and rewrote it as: “Near-surface hourly air temperature, relative humidity, wind speed, precipitation, and shortwave irradiance from observations, uncorrected WRF CTRL outputs, and bias corrected WRF CTRL outputs were compared for the Centennial Ridge, Fisera Ridge, Vista View, Upper Clearing and Upper Forest stations in MCRB, and the comparisons are shown in the quantile-quantile (Q-Q) plots (Fig. 3).”.

Detailed comment 10: Page 6/7, line 31/1: “do not appear to be linear distribution” does not sound to be formulated correctly (linearly distributed). Response 10: Yes, we changed it to “. . .do not appear to be linearly distributed.”.

Detailed comment 11: Page 7: line 6/7: I would suggest to shorten the sentence, for example in this way: “. . . for the uncorrected WRF outputs, with two exceptions: Values of RMSD. . .” Response 11: Yes, we shortened the sentence as suggested by the referee.

Detailed comment 12: Page 8, line 22: unit is missing: 112 mm Response 12: Yes, we added the missing unit.

Detailed comment 13: Page 8, line 26/27: “Sublimation is the total of blowing snow, surface snowpack and forest canopy interception sublimation.” This sentence is either grammatically circular (sublimation is sublimation) or – if the last word does not belong to “blowing snow” - physically incorrect as the blowing of snow means a reduction of snow at the windward site, but by snow transport, not by sublimation in its physical sense. Response 13: Yes, we changed the wording to make more grammatically sound: “Sublimation is the total flux of snow sublimated from surface snowpack and during blowing snow and forest canopy interception processes. . .”.

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Detailed comment 14: Page 9/10, line 31-32/1 and page 14, line 21: Regarding the uncertainty associated with hydrological modelling in general and climate projections, I would recommend to give only one position after the decimal point. Doing so means that you partly lose the differences between the CTRL and PGW values – but that means that they seem to be smaller than the uncertainty. Response 14: Yes, we accepted the suggestion and made changes. “. . . melt rate was slightly higher for alpine ecozone (i.e. from 1.9 mm day⁻¹ in CTRL to 2.0 mm day⁻¹ in PGW) and remained unchanged for forests ecozones (i.e. 0.6 mm day⁻¹ at upper forest and 0.5 mm day⁻¹ at lower forest in both CTRL and PGW).”. “. . . snowmelt rates declined by 1.1 mm day⁻¹ for treeline ecozones and by 0.9 to 1.6 mm day⁻¹ for forest clearings ecozones, but increased by 0.1 mm day⁻¹ for alpine ecozone, . . .”

Detailed comment 15: Page 10, line 23-25: Please explain why the centre of flow volume shifts to an earlier period in PGW, but the peak basin discharge remains unchanged. Response 15: The centre of flow volume measures the 50% of water year flow volume; its shifting to an earlier period in PGW is caused by a combination of earlier snowpack deletion and snowmelt runoff occurrence in springtime and higher evapotranspiration and consequently lower flow in summertime in PGW. For the peak basin discharge, its timing remains unchanged in PGW, while it is at lower value in PGW. The peak basin discharge is balanced by runoff in all ecozones and is primarily influenced by alpine and treeline ecozones at MCRB. While there is no change in peak runoff date for alpine, forest circular clearing north-facing ecozones, peak runoff occurs four days earlier in treeline ecozone but delays by one day to 9 days in other ecozones. This is a complex streamflow generation system with interplay of hydrological fluxes and states from many processes, and in this case, the changes in peak runoff date in all ecozones happens to result in no change in peak basin discharge date.

Detailed comment 16: Page 10, line 32: Please consistently use two positions after the decimal point for the discharge values. Response 16: Yes, for consistency, we used two positions after the decimal point for the discharge values.

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Detailed comment 17: Page 11, line 26 and page 11, line 28: “close” instead of “closed” Response 17: Yes, we changed to “close to”.

Detailed comment 18: Page 12, line 3: “September” instead of “Septmeber” Response 18: Yes, we used the corrected word “September”.

Detailed comment 19: Page 12, line 28: sublimation losses from blowing snow → see comment above Response 19: Yes, we changed wording to “. . . even though sublimation losses from blowing snow in the alpine ecozone and intercepted snow in the forested ecozones also decreased.” to improve clarity.

Detailed comment 20: Figure 2c: Can you please explain the relative humidity values up to 300%? Response 20: Yes, the values of RH up to 300% are the converted values based on uncorrected WRF air temperature, specific humidity, and specific pressure outputs, and we showed the values of RH up to 300% to indicate the errors in these uncorrected WRF outputs. We added some clarification for the values of RH up to 300% in the Section 3.1 of revised manuscript.

Detailed comment 21: Figure 2e/g: The dotted line for “best linear fit” is misunderstanding. It is just the best linear fit for the lower values. For the whole data set, a best linear fit would look different. I would delete this line. Response 21: Yes, we deleted the “best linear fit” line in all plots.

Detailed comment 22: Figure 4: I would recommend to show the simulation line in light blue instead of dark blue to get a stronger contrast to the black observation line. Response 22: Yes, we changed the simulation line to light blue for better contrast.

Detailed comment 23: Figure 11 and 12: The differences between the ecozones would appear clearer if you would use a uniform scaling of the y-axis. Response 23: We tried to have a uniform scaling of the y-axis for both Figs. 11 and 12, but the figures turned out showing the differences among the ecozones, but they are not great to show the differences between CTRL and PGW for some ecozones, particularly when their

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values are small, e.g. Fig. 11c, d, g, and Fig. 12c, d, e, f, g. We included the changed figures with the uniform scaling of the y-axis below, so we think the original Figs 11 and 12 are probably better ones and will keep the original ones. We attached the changed Figs 11 and 12 with this response.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-640>, 2020.

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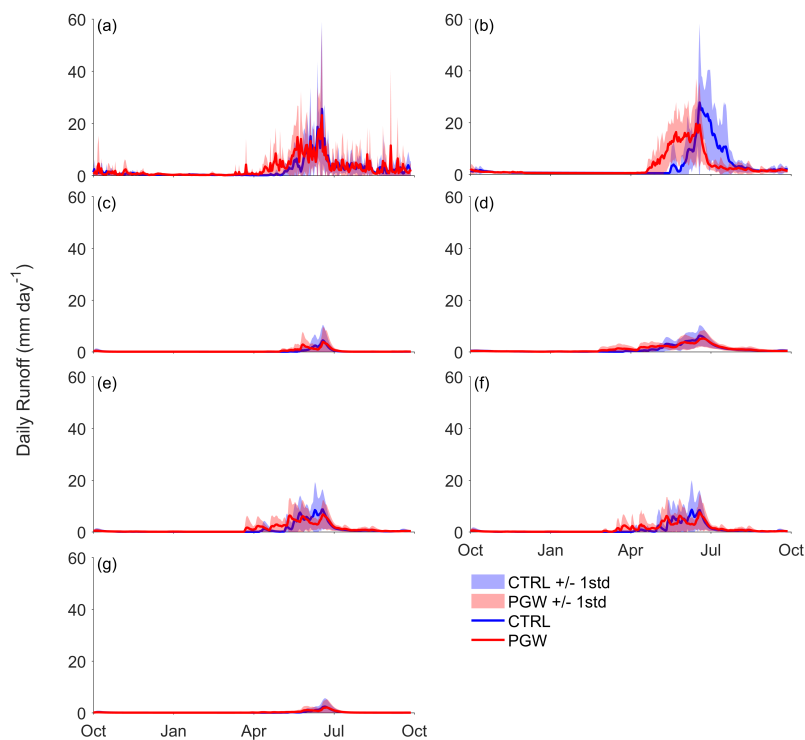


Fig. 1. Figure 11. Simulated annual mean daily runoff for WRF CTRL and PGW. (a) Alpine, (b) treeline, (c) upper forest, (d) forest clearing blocks, (e) forest circular clearing north-facing, (f) forest circular

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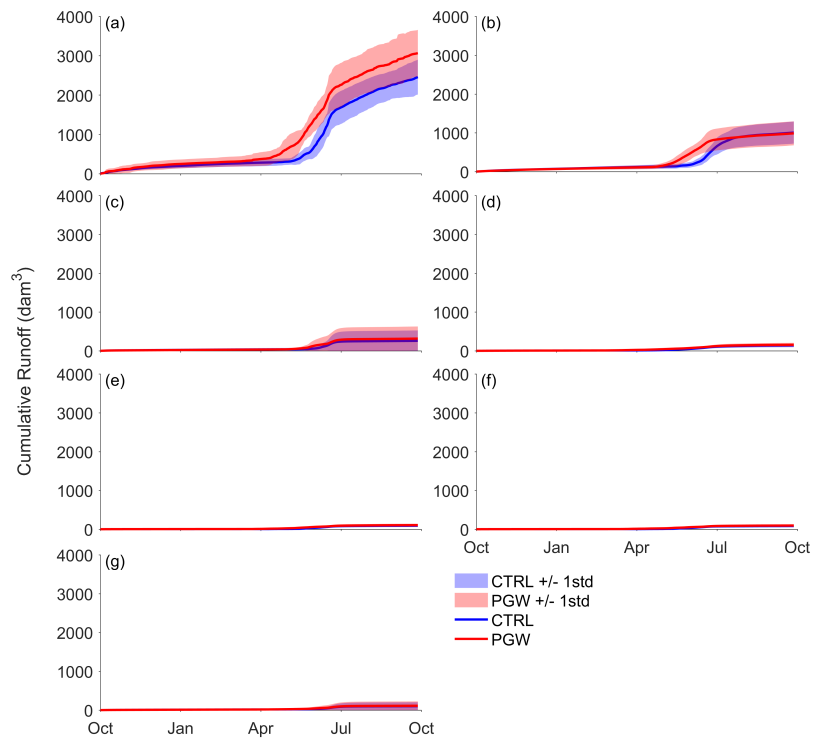


Fig. 2. Figure 12. Simulated annual mean cumulative runoff for WRF CTRL and PGW. (a) Alpine, (b) treeline, (c) upper forest, (d) forest clearing blocks, (e) forest circular clearing north-facing, (f) forest c