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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Examining controls on peak annual streamflow and floods in the Fraser River Basin of British Columbia" *by* Charles L. Curry and Francis W. Zwiers

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General comment

The objective of presented study is to define and analyse major factors which influence annual peak flow (APF) in the Fraser River basin (British Columbia). I think authors did an interesting work by analysing the statistical importance of several snow, meteorological and soil signatures, which influence the occurrence and extremity of APFs in the Fraser River and its tributaries. Besides observations, they used a hydrological model, which enabled to simulate various components of the water balance.



In my opinion, the results are interesting, although not surprising as they mostly support our existing qualitative knowledge of how different natural drivers influence catchment runoff. Although, I did not find many new findings in the paper, I am convinced that the quantification of the role of different drivers on runoff is a valuable and novel contribution. Therefore, I found results of this study important and certainly appropriate for HESS. However, I have some comments listed below, which need to be addressed before I can recommend the manuscript for publication.

Major comments

Although, the introduction section is nicely written, I am missing some larger context coming out from presented studies. Authors were mostly concentrated on the Fraser River basin, but there would be certainly interesting to provide the reader also with larger context by focusing on other regions of the world with similar climates and runoff regimes.

Section 2.3 and Table 2: Authors used three signatures related to rainfall (cold season rainfall, spring rainfall and APF rainfall). However, none from these three signatures reflects the decreasing importance of rainfall amount in time, which might be important when relating these rainfall amounts to APF. Why authors did not use e.g. Antecedent precipitation index API or Current rainfall index CRI (calculated e.g. for the day of year with APF)? Besides, I am a bit confused because authors presented CRI in the result section 3.3.3 and Fig 4, but this index was not included in correlation matrixes (Fig. 3, 6 and 7).

Page 8, lines 22-25: One of the assumption when using multilinear regression (MLR) is that the individual predictors should be mutually independent. How authors dealt with this limitation? I found several times in the text that all possible combinations of predictors listed in Table 2 have been used, but it seems that some of them might be mutually dependent (e.g. spring warming rate and snowmelt rate or NINO3.4 and PDO indexes). Perhaps, the mutual dependence might be also the reason why PDO and

NINO indexes were not selected by the MLR as statistically important.

Sections 3.3.5 and 3.5.5: I am not sure whether the results from the MLR could be somehow practically used. Authors introduced an equation to calculate APF based on selected predictors, but obviously some of these predictors are not known before the start of calculation. Specifically, dT/dt is not known before APF occurs, so it is not much useful to use it as a predictor. Authors are aware of it by mentioning this issue on page 22, lines 9-19. However, I would expect more discussion on this topic. Maybe, authors should not concentrate much on APF prediction in some specific year, but they could rather discuss how future changes in predictors (e.g. future increase in air temperature) might influence the APF in the future. I am aware that such discussion might be a bit of speculative manner (since the future changes in predictors was not the scope of the paper), but it could still bring some new insight on results.

Overall, I am a bit uncomfortable with the paper structure. Authors sometimes mix methods, results and discussion together. Some methods are described in the results section (e.g. current rainfall index in section 3.3.3; dealing with “glacier cells” in section 3.4). Similarly, there are a lot of discussion text in the results section. Therefore, “Discussion and conclusions” section is just a repetition of major results without any discussion. I suggest to better separate the individual parts. In my opinion, authors could either make a separate section called “Discussion” and put all related text into it, or (which might be an easier solution) they could make a section “Results and discussion” where they can put all related information. I do believe that this way the text will be clearer to the reader.

Minor comments and technical corrections

The abstract sounds a bit vague to me. Maybe, authors could present their results in more detail (some numbers etc.).

The aims of the paper are not explicitly defined in the introduction section (although, they are intuitively deductible from given context).



Page 3, line 5; page 15 line 28: I think “On the other hand” is not correctly used here since there is no “on the one hand” before.

Page 4, lines 11-13: I fully agree with this statement. However, I did not find any section where authors discuss this issue within the context of their results (maybe, I just missed it).

Page 6, line 4: Undefined abbreviation (WSC).

Page 6, lines 8-10: I think there should be more explanation about the mentioned 19 locations. Were they equally distributed in the basin area? Do they cover whole elevation range? This should be shortly mentioned in the text.

Page 6, line 12: The SST is defined only later (line 15). This abbreviation should be defined already here.

Page 6, line 18: Missing year in reference Gurrapu et al.

Section 2.2: For those, how are not familiar with VIC model, I would expect at least a brief introduction of model routines (specifically, snow and soil routines since they seem to be most important for analyses presented later in the paper). It might help readers with understanding such they do not need to find information about the model in other literature.

Page 7, lines 10-17: The calibration and validation procedures should be described. Which objective function(s) has been used? Was only one “best” parameter set used for further simulations or more parameter sets were used to decrease the uncertainty resulted from calibration? This should be clarified. Additionally, it was not fully clear to me, whether authors did the model calibration by themselves or they used simulations done by someone else. It seems that latter is true, but it is not fully clear from the text.

Page 7, line 8: What exactly means “computational constrains”? Please, specify.

Page 7, line 14: “...were in better agreement with observation...”. Based on which criteria?

Page 11, lines 17-20: The procedure describing how to compute CRI should be placed rather in methods section.

Page 13, lines 27-33: Why model simulated mentioned unrealistic SWE in high-elevation grids? I am not familiar with VIC model, but unrealistic increase in SWE at highest elevations (sometimes called as "snow towers") exists also in some other hydrological models and this problem is usually connected with simplified degree-day method used in snow routine. Typically, when 1) snow routine uses only one value of melt factor over the whole melt season and thus it underestimates the radiation inputs and/or 2) snow routine does not account for snow redistribution due to wind.

Page 14, lines 1-5: I think this paragraph should be placed in methods.

Page 15, lines 8-9: Could dT/dt explain the mentioned large APFs occurred during average snow conditions?

Page 15, lines 22 and 23: missing values of p.

Page 18, line 6: Maybe, I would not title this chapter as "Case study" since basically everything presented in this paper is a case study. Thus, I suggest to simply title it "3.8 High-flow years in the FRB".

Table 2: I would prefer "Day of year (DOY)" instead of "Julian day" since the Julian day is the continuous count of days since the beginning of the Julian period.

Table 3: I guess there should be "APF" instead of "APDF".

Figure 1: Missing scale. Additional, the colors in the color scale are not appropriate for DTM (red to blue is rather used for air temperature).

Figures 2, 3, 5, 8 and 9: Maybe, the size of captions (axis, legend) are too small.

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