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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

Anonymous Referee #1

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Overall, this is a careful and interesting statistical study of what controls the magnitude of streamflow in a particular large river basin. While there doesn't seem to be anything technically wrong with the analysis, I feel that the paper can be improved significantly by considering the following points:

Major comments:

1. Given the emphasis on streamflow "predictors" and the explicitly indicated application of seasonal streamflow prediction (e.g., in the abstract), it seems to me that the study would be much more useful if different sets of statistics were computed for different prediction lead times. For example, on the face of it, it doesn't make much sense

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to consider an SST index based on a previous season's SST conditions (p. 6, lines 10-14) along with a warming rate (p. 10, line 16) that apparently can only be computed on the day of the APF itself; if one were to wait so long into the high streamflow season to estimate APF from various variables, mightn't they use concurrent SSTs, which may have changed significantly? Another example: the multiple regression (equation 1) connects SWE on April 1 with a heating rate (dT/dt) that uses information up to the date of the APF. Someone interested in predicting subsequent APF given May 1 conditions presumably would want to use May 1 SWE and the heating rate in the period leading up to May 1 as the predictors (along with the average multi-month temperature up to May 1). Multiple correlograms could in fact be constructed, based on a selection of different forecast start dates (i.e., based solely on information available on those start dates). Such an expanded set would be more informative and intuitively more understandable than the single set provided here, given that the single set draws on information at such different leads.

Perhaps the main usefulness of the study is not to predict APF but (as also mentioned in the abstract) to project future APF behavior based on projected changes in the different predictors. That may be, but then some discussion is presumably needed about how well one can possibly project such changes in the predictors. We have a hard enough time convincing people that we can predict precipitation changes at the basin scale, let alone the more specific predictors discussed here.

2. The authors note the potential for soil moisture state to influence streamflow but don't include it in their calculations for the observational analysis (section 3.3) due to the unavailability of data. Why not use, as a surrogate, the average fall rainfall or summer+fall rainfall prior to the (estimated) time of soil freezing?

3. The case studies are fine, as far as they go. The authors should remember, however, that the typical reader is not specifically interested in the Fraser River Basin; they are reading the paper for a more general understanding of what controls streamflow magnitudes. In my opinion, the case studies don't add all that much; they could be

removed to make room for the expanded analyses suggested in Comment 1 above.

Minor comments:

– Table 3: Change “APDF” to “APF”.

– p. 11, line 9: How can APF be said to depend on R_APF, when the latter includes information subsequent to the date of APF? A confusing variable to consider (though I don’t think I see it in the correlogram, anyway)...

– The point of Figure 4 is not as intuitive as the authors assume. More description is needed to describe the whole CRI thing.

– p. 15, lines 16-17: How does the recharging of soil moisture increase further melting? This doesn’t make sense.

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