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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes" *by* A. E. Coles et al.

Anonymous Referee #2

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The authors use a decision tree method and a 52-year data record of snowmelt related runoff from three agricultural plots in the Canadian prairies to study the controlling factors on runoff generation over seasonally frozen soils. Spring runoff conditions are characterized by determining runoff ratios. Decision trees are then used to understand the relative importance of a number of control parameters describing topography, land use, vegetation, and precipitation dynamics on the spring runoff. The intent of the study is to improve the understanding of the relative importance of the factors responsible for runoff over frozen grounds which should help to improve models describing infiltration into frozen ground and runoff over frozen hillslopes. The authors also want to give some advice to future measurement campaigns looking at seasonally frozen, snowmelt dominated regions. The authors claim that the major innovative part of their study as compared to other studies looking at runoff generation cover frozen ground

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is, that those studies usually are based on short term experiments and single season runoff events, while their study looks at averages over a much longer data period. This longer record should help to reveal the nonlinearities and interactions between the various process controls. In my opinion, this aspect of the study could indeed present some important new insights into the scientific field of snowmelt runoff generation over frozen ground. The paper is generally well written and the language and citing are appropriate. However, I find parts of it (especially in the Methods section) rather difficult to follow (see comments). The introduction and literature review focuses very narrowly on studies mostly from western Canada. While this is understandable due to the study location, I think a broader look at studies of runoff over frozen soils would be beneficial. The research goals on the other hand are clearly stated and well defined. The results and discussion section are adequate but could maybe be structured a little more clearly (see comments). The conclusions are based well on the results and provide a good summary of the study. The Tables are concise and easy to interpret, while some of the Figures could be improved (see comments) to make them easier to grasp. The topic of the article falls within the scope of the journal and does in my opinion represent a worthwhile contribution to the snow science community. However, the article needs in my opinion some fairly substantial changes to make it less difficult to follow. I would therefore recommend publication after major revisions (see general and specific comments).

General Comments

As mentioned, the literature review in the introduction should be broadened to include at least some studies of runoff and runoff generation over seasonally frozen or even permafrost soils. I would also welcome a discussion of the relevance of the studies result for other regions of the globe. Are the study results only applicable to prairie landscapes or could similar hierarchies and interactions be found for example in high arctic locations that have frozen soils and often similar topography. This discussion could also be included towards the end of the paper in the general discussion section.

I found it somewhat hard to follow the explanation of the decision tree method in the Methods section. The description is very technical, which makes it hard for the average reader to understand what the analysis really means. I think it would be beneficial to include some explanations as to what it means f.e. if some predictors occur several times in a tree, what the ranking of the nodes mean for the importance to runoff generation, and what a leaf really represents “in reality”. I guess I would recommend to “dumb down” the explanation a little bit so that readers who are not familiar with the decision tree technique can appreciate what the single tree elements (nodes, leafs etc.) mean for the description of the “real life” process. I would really encourage a much more in depth presentation and discussion of the predictor variables as part of the discussion. The choice of which predictor variables to use is a major point of the study and should be treated as such. So far information on the predictors is only available within Table 2 and is very short. I would especially welcome a short description for each predictor, why they were chosen, and how they theoretically are expected to influence runoff. Additional points that could be discussed here are: What processes are believed to lead to the differences between total snowfall SWEf and snow cover SWEc. How closely are these two predictors related (maybe include an autocorrelation analysis). What impact does the soil fall water and fall soil profile water content have on the mechanics of spring infiltration (i.e. formation of concrete ice soils when water content is high, this might not be obvious to readers not that familiar with frozen soil hydrology)? Why is mean daily wind speed needed? The process deposition or removal of snow from the test plots due to blowing snow should be included in the difference between total snowfall SWEf and snow cover SWEc. So what additional information or impact does blowing snow during the winter have on spring runoff. The authors use 4 predictors to describe melt conditions. Wouldn‘t melt season length and melt rate be enough? What additional information do the other two predictors provide?

Specific Comments

p.4 Line 5 and following The authors describe the usual conditions on the test plots

as they were implemented in the decision tree analysis and the exceptions to those conditions. Unfortunately, considering the study period of 52 years there seem to be quite a lot of extraordinary conditions considering land use and tillage. One would assume that especially different tillage practices and the presence of stubble or standing crop over the winter (additional catch of blowing snow as mentioned later in the paper) could have quite an impact on runoff. Yet it seems like all years were included in the analysis. At the very least I would expect a discussion of the possible impacts of this. p.6 line 25 The runoff ratio is defined as total runoff divided by SWEf from the end of the hydrological year to the end of snowmelt. How was liquid precipitation (i.e. rain) handled in this? I'm aware that in Saskatchewan rain after the end of the hydrological year and before the start of the snowfall and during snowmelt is rather rare, but it must have occurred sometimes during the 52 year study period. Also how was SWEf measured at the met station (precipitation sampler, snow depth on ground)? p.11 Section 4.2 This is a very minor point. Here the authors present the results from the secondary decision trees that split the dataset into high or low expressions of the six key variables. The results are presented in Table 5. The results start with a discussion of high and low snow cover years and then moves on to high and low total snowfall. These two variables are presented in reverse order in Table 5. You might want to change either the Table or the results section. p.12. line 5 The authors describe that the top three controls on runoff ratio matched the overall hierarchy for high snow cover years, albeit with differing orders of importance. Maybe you could quickly include that changed order so that the reader does not have to look for Table 5 to find out what that order was. p.12 line 11 The authors state that for low fall soil surface water content the analysis indicated that mean daily wind speed had a large influence on runoff ratios. Could you maybe discuss why this could be the case. The connection is not clear to me at all. p.16 Figure 4 This Figure is very difficult to understand. It needs to be explained in much more detail. Also the inserted panel in the right hand figure showing Figure 3.3. (why 3.3?) is way too small to be readable. I would recommend removing it entirely. The left hand panel of the Figure is not explained in the text at all and the rudimen-

tary legend box is not enough to make it understandable. P17. Line 29 The authors state once again that the six key variables explain most of the variability and exert the greatest control on runoff ratio. Maybe one could add a discussion here of whether including only these six variables is “good enough” for a model or how much additional information the other 9 used parameters really provide. P18 Figure 6 Intuitively I would have expected the wet and dry scenarios in panel D to be presented in different order, i.e. wet on top and dry on the bottom. p.18 end This sentence is virtually identical to a sentence on p. 16 lines 12 and following p. 20 line 14, 15 If the relationship was so strong, why wasn’t length of frozen soil over the winter not included as a predictor? Please add explanation.

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