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Discussions

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

Anonymous Referee #1

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The manuscript aims at identifying the relative importance of controls on snow-melt runoff from three arable plots of approximately 5 ha located in the Canadian Prairies. To this end they use a statistical method – decision trees – with a set of 15 predictor variables to analyze nearly 50 years of runoff data. The authors conclude that the relative importance of controls is not general for all winters, but changes (for example) with the thickness of the snow cover.

As the authors correctly show in the introduction, there has been a substantial amount of experimental studies in the past 30 years on winter-time runoff generation with a focus on the snow cover and the frozen soil – at different scales (from small plots to catchments) and in different regions of the world (incl. Canadian Prairies, Northern Scandinavia, Alpine areas and Japan). So, I dare say that we know quite a lot already about important controls of snow-melt infiltration and runoff – including formation and

permeability of a frozen soil layer. Therefore my first reaction when I started reading this manuscript was: do we really need a statistical analysis to find out the "hierarchy of controls" on snowmelt-runoff for seasonally frozen areas? And, what's the added value of such a statistical analysis to the existing knowledge from empricial studies and deterministic modelling? After having finished reading the manuscript I have the feeling that the lessons learned from this exercise are marginal. This has to do with a number of critical drawbacks of this study:

a) The number of winter runoff situations (response variable) is critically low compared to the large amount of predictors (15).

b) Some of the predictor variables are highly correlated with other predictor variables.

c) We know from many field experiments that runoff in spring is not the result of average winter conditions, but can reflect critical short situations of the winter; e.g. short mid-winter melt events, or short coincidence of a shallow snow pack and very cold air temperatures generating substantial soil frost. This applies in particular to regions with a broad range of soil frost conditions, such as the Canadian Prairies, that are sensitive to the snow-cover thickness. In conclusion, an analysis based on important predictor variables representing mean winter conditions (e.g. mean temperature or total seasonal snowfall) is probably not very conclusive.

There are a few other issues with this study that I consider as critical:

- The title and some parts of the manuscript imply that the relative importance of the different controls on snowmelt-runoff generation – found in this study – are general. But in fact, it only applies to the specific slope, soil and meteorological conditions of this field site. How can the hierarchy of controls found at this site be transferred to other sites with other snow and soil conditions, or with another topography?

- The reader gets very little information about the specific conditions of this site. In fact, it seems that what is called "snow-melt runoff" is only "surface runoff". What about lateral discharge? Is there a groundwater table fluctuating near the surface? What do we know about the soil type and the soil physical properties? How does a typical snow cover look like in this area? I guess it must be very patchy and loose. What's the typical length of the winter season? Such information would be essential for interpreting the results.

- Two of the most important predictor variables are "Total seasonal snowfall" and "snow cover (i.e. peak SWE)" (see Fig. 3) – (which is by no means a surprise) – and I assume that these two variables are highly correlated. How do the authors justify the selection of these two predictor variables?

- One key-variable for snowmelt-runoff generation at this site is completely missing in the analysis (and also in the discussion!): the extent of the soil frost. And with that I mean both the frost depth and the ice content. I didn't find any information about typical frost depths, for example. Of course, there is an implicit account of soil frost because of the strong (negative) relationship between snow cover and soil frost depth. And there is an implicit relationship between pre-winter soil water content and ice content later in the winter. But this obvious key-connection is not made.

- How about land-use? According to chapter 2 the three plots were covered with different vegetation and experienced different tillage practices. But it seems that this was not of major importance for the snowmelt-runoff generation. (Also the topography of the three slopes is more or less the same.) If that's the case, I think that the decision tree actually has only an N of 52 (and not 140), which makes the statistical analysis even more questionable. However, if vegetation and tillage have a significant impact, then it should be also discussed somewhere.

- Chapter 5.3: Implications for modeling and future field campaigns: are we going to change current practices in modelling snow-melt runoff (in regions with seasonally frozen soil) based on the lessons learned from this statistical analysis? I don't think so. The key-role of the snow cover and the significance of pre-winter soil moisture content

has been known for quite a long time and is accordingly represented in current runoff models. And also the critical need for accurate and spatial snow-cover data is well recognized.

- Finally, the world-wide knowledge on snowmelt-runoff generation in areas with seasonally frozen soil is not well reflected in the introduction. Only Canadian studies are referred to.

In conclusion, I'm not convinced that the above-mentioned problems can be solved with the available data and the selected method to become a contribution that adds value to the existing state-of-the-art knowledge on snowmelt-runoff formation in cold regions.

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