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

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Author responses to Reviewer #1's interactive comments on: Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-564, 2016 "The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes" by A. E. Coles et al.

Authors:

Summary

Thank you to the three reviewers (Reviewer #1, Reviewer #2, and S. Schälchli) for their in-depth reviews of this manuscript. Your reviews are very useful in improving this manuscript. We reply to each of the comments in separate documents. But we wanted to provide a brief summary here of what we saw to be the six main comments on the

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Discussion paper

manuscript (some highlighted by more than one reviewer) that we will improve in the revised version:

1. Literature review. We will include more descriptions of what existing knowledge tells us about how each of the predictor variables individually influences runoff. We will extend our literature review to outside of the Canadian Prairies.

2. Transferability of findings. We will add more of a discussion on how these findings are transferrable to other regions.

3. Methodology. We will add more description of how the decision tree elements can be interpreted. We will make the methodology section less technical.

4. Predictor variable correlation. We will remove some of the predictor variables that correlate with others, and restructure the decision tree based on that.

5. Land cover and tillage. We will add tillage to the decision tree analysis, and add more discussion about land cover and tillage effects on runoff.

6. Implications for modelling. We will provide more discussion on specifically how this decision tree analysis can improve modelling approaches, such as statistical 'add-ons' to existing empirical approaches.

We talk about these six aspects in our responses, as well as several other comments that the reviewers had.

Below are the comments from Reviewer #1 and our responses to those comments.

Reviewer #1: 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.

Authors: Thank you for your comments. It is unfortunate that you feel that the lessons learned are marginal and that we haven't properly detailed the added value of this statistical analysis. We do think this analysis has provided added value. We agree with you that we already know a lot about the important controls on snowmelt-runoff, and we refer to these up front with previous research in the Introduction section. We will rework the literature review to better summarise what is already known mechanistically about the important predictors on snowmelt-runoff (while also expanding this to other, non-Prairie studies on snowmelt-runoff).

Despite our existing in-depth knowledge, however, there are persistent problems with modelling snowmelt-runoff. This is likely down to the fact that runoff generation and runoff ratios are strongly spatially and temporally variable. At one site, the runoff ratio of the springtime snowmelt could vary hugely from one year to the next. While individual controls and their influence on runoff are well understood, we believe that this high variability in runoff is in part due to interactions – sometimes subtle – between controls, and shifting relative importance depending on other conditions. We believe that teasing apart these interactions and hierarchies of controls – especially the condition-dependent hierarchies – is the first step in designing more effective monitoring schemes, and incorporating the right parameters together in empirical models.

Reviewer #1: 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).

Authors: For the three hillslopes, we have 140 runoff ratios between 0 and 1. For each of these runoff ratios we had a set of 15 predictor variables. This is enough observations to allow for a decision tree approach. As explained in the manuscript, we believed it be overly pedantic and physically unrealistic to attempt to predict runoff ratio values (e.g. RR=0.55 given a set of predictors). It was more appropriate to group the runoff ratio responses, and we settled on 5 classes for this, and instructed the decision tree model to predict the runoff ratio class.

We agree that the large number of predictor variables means that the approach is at risk of model over-parameterisation and overfitting. However, we took steps to avoid this by using a technique called pruning. This meant that not all predictor variables (only 2 - 6) were used in any one decision tree.

Reviewer #1: b) Some of the predictor variables are highly correlated with other predictor variables.

Authors: Yes, we ran a correlation analysis. Snowfall and snow cover are significantly correlated ($p<0.05$), as are fall soil surface water content and fall soil profile water content. You're right that they both should not be included in the analysis. We will remove snowfall and fall soil profile water content.

Reviewer #1: 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 midwinter 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.

Authors: We agree! We are limited somewhat since each predictor variable must constitute only one value for each response variable (seasonal runoff ratio), therefore some conditions can only be captured by averaging or summing over a period. However, the only predictor variable that used a general winter condition was 'total seasonal snowfall'. Our other winter-related predictors attempted to get at these critical short situations of the winter. For example, 'mean temperature on warm winter days' is not mean winter temperature but instead is the mean temperature of days which had air temperature that exceeded 0°C AND coincidentally had snow cover on the ground (i.e. an indicator of potential over-winter snowmelt event). A second example is the 'mean daily wind speed above blowing snow threshold', which is the mean wind speed for days where winds exceeded 7.5 m s-1, which is the threshold for blowing snow redistribution. Thus, these are attempts to obtain metrics of these specific conditions, while encapsulated in a single value, and aren't just a generalised winter condition.

Reviewer #1: 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?

Authors: You are right in that the analysis is based solely on conditions at this study site. We will refocus those parts of the manuscript that inappropriately imply that they are general to all snowmelt-runoff systems. We will also add a discussion section on transferability to other sites. Although only one site with three hillslopes is studied here, we believe that it enables transferability since, with such a long dataset, we have observations of multiple snow and meteorological conditions, multiple soil moisture conditions, multiple frozen ground conditions, and three topographic realisations. So, in a sense we are substituting space for time. We would suggest that limits to transferability are the soil type (silt loam) and agricultural nature, since these are two aspects that are unchanging through the study period.

Reviewer #1: - 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.

Authors: Thank you for pointing out that we need more site-specific information. We will include much more detailed information in Section 2 (Study site and dataset) concerning the aspects that you point out. Briefly here, yes runoff from the hillslopes is from surface runoff. The groundwater table is several meters below the soil surface, and does not fluctuate near the surface. The soil is a silt loam (we already state this in the manuscript), with very low infiltration capacities when frozen (0.09 to 2.57 mm hr-1, which we observed in laboratory experiments). There might be small amounts of lateral flow after thawing, but it is reasonable to assume that is unimportant. We will also include bulk density, soil depth, and unfrozen infiltration capacity data. No, snow cover is not patchy and loose. Typically, the site is 100% snow-covered over winter and into the spring snowmelt season. End of winter hillslope-averaged snow depth is up to 40 cm (much deeper in topographic depressions, shallower on exposed areas), while snow density averages 0.24 g cm-3 and can be up to 0.45 g cm-3. The winter season is typically 4-5 months in duration, when up to 150 mm precipitation falls as snow. We will incorporate all of this information into the revised manuscript.

Reviewer #1: - 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?

Authors: Yes they are correlated. We had previously included both since the decision tree's selection of one would tell us which held more predictive power. However, as mentioned above, we now suggest that we should remove snowfall due to the correlation. In its place, we would bring in a new variable that indicates over-winter ablation (e.g. the difference between snowfall and snow cover).

Reviewer #1: - 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.

Authors: We disagree that it is missing! We included what we called 'thawed layer depth' as a predictor variable, which is the depth between the soil surface and the top of the frozen layer on the peak runoff day. It is an indicator of whether runoff was occurring over ground frozen right to the soil surface, or over ground that was thawed. Typically, the ground is frozen to the soil surface ('thawed layer depth' = 0 cm) at the time of peak runoff. Given that this system is a surface runoff-dominated flow regime, this measure of the upper boundary of frozen ground is important. You are right that the pre-freeze soil water content (fall soil water content) gives an indication of the frozen ground ice content. Apologies if this connection was not made clear in the manuscript; we will make sure that it is clear in the revised manuscript. We do actually discuss at length the reasons why frozen ground ice content might vary from pre-freeze soil water content (most notably in Section 5.2 on 'Soil moisture memory'). As for the soil profile depth to which frozen conditions typically extend (the lower boundary of frozen ground), we will add information on typical values for this in the study site (along with the information on soil depth, bulk density, infiltration capacity, etc.). We also discussed observations that longer periods of deep soil freezing (at depths of 50 cm and 100 cm) is positively correlated with runoff ratio in the following spring (page 20 lines 14-21). We will run a correlation analysis to assess the correlation between the depth of frozen soil at the time of runoff and snow cover depth (SWEc). If there is no significant correlation, we will include this as a predictor in a revised decision tree analysis.

Reviewer #1: - 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.

Authors: We described findings that showed fallow hillslopes had generally higher runoff ratios than other land cover types (page 15 line 25-27). We also outlined results of the effect of standing stubble on snow retention, and its potential knock-on effects on soil water content and runoff ratios (page 15 line 27 onwards). We included land cover variable in the decision tree analysis, but it was rarely (just once) picked out as an important control on runoff. We suggest that this is because soil moisture and snow cover are the avenues by which land cover influences runoff (vegetation effects on soil moisture depletion, and snow-trapping effects from crop residue), and therefore it is those factors that are picked out by the decision tree rather than land cover directly. We can include more discussion on the role of vegetation, and also include tillage in the analysis, in the revised manuscript.

Reviewer #1: - 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.

Authors: There are several empirical approaches that estimate infiltration into frozen ground that use two parameters: soil moisture and SWE. We tested one of these – the Granger equation, which is a widely-used model – and showed that the two-parameter model does not work as well as the approach that we have presented in this paper. While, yes, the influence of the individual controls has previously been shown extensively, here we were able to show with a large dataset the interactions and hierarchies not usually apparent.

We propose that we will cut down the paragraph on page 22 lines 11-26 to make it more concise. We will add discussion on how existing empirical approaches can be improved with the findings of this study. One option could be to design an error model based on the Swift Current data that accounts for the variability captured by these extra parameters, and apply it as an add-on to the existing Granger empirical equation.

Reviewer #1: - 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.

Authors: Yes, you're right. We will incorporate more studies from outside of the Canadian Prairies.

Reviewer #1: 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.

Authors: We believe that we have addressed the comments that you have made. We agree that more work can be done to improve this manuscript, and we know the ways we will do this. We think that this rare, long-term, multi-variable dataset and the decision tree method do actually contribute a great deal to extending our knowledge on the interactions, feedbacks and condition-dependent hierarchy of controls on runoff.

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