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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.

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Author responses to S. Schälchli'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

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 S. Schälchli and our responses to those comments.

S. Schälchli: Review

The article includes many very interesting contents and also adds new results to the broader scientific context. Furthermore they came up with new ideas and used tools, which were never used in this research context before. The Title reflected the contents of the paper clearly.

In the beginning the article was quite hard for me to read. I had to read some paragraphs twice and look up some technical terms. Especially the abstract uses, from my point of view a quite difficult language and the theme often jumps among the different results.

Authors: Thank you for your review and helpful comments. We will make sure all technical terms are defined. We will edit the abstract to improve the flow of results.

S. Schälchli: The Introduction was written clearly and the authors made a good assumption of the present scientific stage. Furthermore they linked their research to other papers and declared why their research topic is of relevance. They generated a very good embedding of their topic into the broader scientific range. The research questions were pointed out clearly. But the 3rd Question was answered insufficient, because the author didn't mention in what way the hierarchy varied from year to year.

Authors: You're right. We neglected to update the terminology of the research questions. We will edit 'year to year' to 'condition-dependent' variability in hierarchy.

S. Schälchli: The used data was in the majority good, but as they also made clear, the measurements of the soil water content were made a bit too seldom. The measurements have been done in October (before the freeze-up) and then only in April when the melting process has already begun in many years. This is not an ideal presupposition for the correctness of the soil water content and therefore for the whole influence of the soil moisture on the runoff output. The reason is that until April there may occurred infiltrations (rising of soil water content) or percolations (decrease of soil water content).

Authors: Yes, we discuss this in the paper. The soil water content measurement in the spring was always carried out after the end of the spring snowmelt season. Therefore it absolutely does reflect any change in soil water content over winter (increases due to mid-winter ablation events or upward migration towards the freezing front, or decreases due to deeper percolations) as well as increases during the spring snowmelt season.

We detail this clearly. It is unfortunate that we do not have pre-melt soil water content data. A recommendation of this paper is the importance of observing pre-melt soil water content to account for any over-winter changes in the soil water content.

S. Schälchli: Another question crossed my mind when I read the chapter 2 on the study side and the dataset. Is there maybe a difference in the snow adhesion by the diverse crop types stubbles? For example in term of the stability respective to wind redistribution?

Authors: Yes, we show that on p.15 line 29-31 that wheat stubble (which occurs when a wheat crop year coincides with no tillage in the fall, to leave standing residue) retains more snow cover than, for example, fallow, due to the snow-trapping qualities of standing stubble and less redistribution by wind. While other vegetation (grass, lentils) do leave 'residue' on the hillslopes if not tilled, they do not have the same snow-trapping qualities as standing wheat stubble.

S. Schälchli: I have to point out that I wasn't completely able to follow up with the methodology part. For example it was unclear to me how the authors came up with the decision in which hierarchy the controls are in an explicit case and I also had my difficulties to read the decision tree. First when I read the whole paper trough and then returned to the abstract or to the decision tree the intentions became clearer.

Authors: Thank you for that feedback. Reviewer #2 also found the methodology and decision tree elements difficult to understand. We will work on the methodology section and improve its clarity, with focus on how the hierarchy was decided and how the decision tree elements can be interpreted.

S. Schälchli: The results and the conclusions were written very comprehensible and contained many good visualisations and graphics. The Figure 4 and 5 were in particular very illustrative and informative. However why there is a sharp step of the seasonal snowmelt-runoff at 70 mm is unclear to me. Their results were highly interesting, especially in terms of the influence of the soil water content under the seasonal frozen

conditions on the runoff output. I support their appeal to measure also the pre melt soil water content (Coles, A. E. et. al. (2016): 23). Than during reading the results I was thinking about the faults which may occur when the soil water content is only measured twice a year and thus was very happy to find my thinking's reinforced in the conclusion part by the authors themselves.

Authors: Thank you for your feedback about the source of the 'step' in Figure 4 being unclear. The step is at 71.5 mm for total seasonal snowfall (x-axis). We incorporated that into this figure because the decision tree (Figure 3) found that total seasonal snowfall, with this threshold amount (71.5 mm) to be the first control (the first node in the tree) on determining runoff ratios. Since Figure 4 was a partitioning of the relationship between total seasonal snowfall and total seasonal snowmelt-runoff, we thought it made sense to break it up according to the first branching of the decision tree. However, we appreciate that this is a very definite line, while the partitioning ought to be more 'fuzzy' given the <100% predictive accuracies and multiple combinations of variables that dictate a runoff ratio. We will edit this figure to remove this step, and exhibit a more 'fuzzy', less definite partitioning of the relationship between snowfall and snowmelt-runoff.

S. Schälchli: Finally the conclusions were hold shortly and precise with a clear statement addressing further research on the influence of soil water content in seasonal frozen soils on the runoff output.

Authors: Thank you. We're happy you found them to be precise and clear.

S. Schälchli: Recommendation List (bold = major points, italics = minor points): âĂć Central theme in abstract have to be found and a better organisation of the results (in Abstract) should be done âĂć 3rd Research question: Variations in between of the different years were not addressed âĂć Method part should be better clarified and the functioning of the decision tree should be explained better âĂć Figure 4: Author should explain the occurring sharp step at 70 mm in the manuscript. And are the shapes in the soil boxes the granularity of the soil or ice lenses? Label it clearer. âĂć Comparison of

the different crop types and their effect on snow cover and runoff âĂć Interesting would be how the determining factors changed over time and if there is a trend in the data (maybe caused by climate change?). If there is a trend: How does the change in the determining factors influence the snowmelt runoff?

Authors: We have addressed most of these comments in our responses above, and will edit the manuscript accordingly. We will edit Figure 4's legend to make that clearer. Regarding the trends in the data, long-term climate and runoff trends at this site was the subject of another study (Coles et al., 2017), which we will refer to in the next version of this manuscript.

Thank you again for your review of our manuscript!

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