Review of the research paper "The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes – by Coles et. al."

Summary

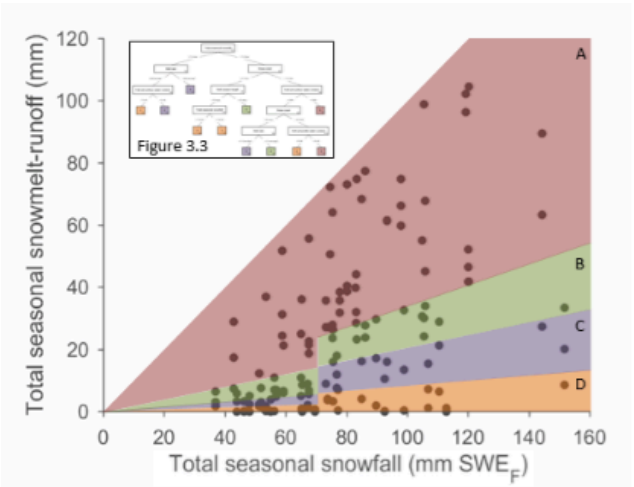
Schälchli, S.

Coles et. al. used a 52-year dataset from a research on a farmed, undulating hillslope in northern Canada which contained multiple measurements of meteorological, runoff, soil water content and snow cover parameters (Coles, A. E. et. al. (2016): 3-6). They were interested in all these determining factors, which influence the snowmelt-runoff. Their target was to find out "the hierarchies of controls on snowmelt-runoff generation in seasonally-frozen, snowmelt-dominated region, and any interactions and feedbacks between those controls" (Coles, A. E. et. al. (2016): 3).

For implementing their research questions they used decision trees to identify the hierarchies in the influencing variables. For the construction of the decision tree Coles et. al. used the CART algorithm of MATLAB (Coles, A. E. et. al. (2016): 6). They defined 5 different runoff classes (where as the first was the lowest runoff). By splitting the first decision tree, which contained the whole information, by the median they were able to explore the causes for the low or high runoff ratios under opposite conditions. They defined several determining factors, which were tested on their hierarchical influence on the snowmelt-runoff and under which conditions which factors were dominant (Coles, A. E. et. al. (2016): 7-9).

Coles at. al. found out that in this long term analysis data show little connection between precipitation and total seasonal runoff output (Coles, A. E. et. al. (2016): 9). This indicates the immense nonlinear relationship on seasonal frozen ground between precipitation inputs and runoff outputs (Coles, A. E. et. al. (2016): 16). The main factors which influenced the runoff output were the total snowfall, snow cover, melt rate, fall soil water content trough the whole soil profile and at the surface and as well the melt season length. All this factors together accounted for 70 % of the runoff ratio variance (Coles, A. E. et. al. (2016): 9-10).

Their main finding in their study was that the fall soil surface water content was mainly important in direction to the snowmelt-runoff (as the following figure 1 shows) (Coles, A. E. et. al. (2016): 16).



Coles, A. E. et. al. (2016): 16

They recognised that especially when the snow cover was high the soil surface water content was the mayor control for the snowmelt-runoff in spring. Reverse when we had just small snow cover during the winter season the fall soil surface water content was not so important for the runoff. This is because in this case the runoff output is mainly controlled by spring related controls as melt season length and the peak date of the runoff (Coles, A. E. et. al. (2016): 12-15).

Summarizing we can say that the soil moisture content is very important for the snowmelt-runoff in seasonal frozen hillslopes (Coles, A. E. et. al. (2016): 23). Just because of the high importance that soil moisture content (before the melting season) have on the prediction of runoff outputs, it is particularly important to observe the still existing uncertainties concerning the influence of infiltration rates into the soil during winter. Thus in further studies the scientist should also measure the soil water contents (on surface and in the hole profile) during winter months to determine the influence of ablation events as sublimation, melting or snow redistribution by wind (Coles, A. E. et. al. (2016): 16-20, 23).

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.

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.

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).

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?

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.

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.

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.

Recommendation List (red = major points, green = minor points):

- Central theme in abstract have to be found and a better organisation of the results (in Abstract) should be done
- 3rd Research question: Variations in between of the different years were not addressed
- Method part should be better clarified and the functioning of the decision tree should be explained better
- 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.
- Comparison of the different crop types and their effect on snow cover and runoff
- 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?

Literature

Coles, A. E. et. al. (2016). The hierarchy of controls on snowmelt-runoff generation over seasonally-frozen hillslopes. Hydrol. Earth Syst. Sci. Discuss. In Hydrology and Earth System Sciences HESS. pp. 1-27.