

***Interactive comment on* “Characterizing the nature and variability of avalanche hazard in western Canada” by Bret Shandro and Pascal Haegeli**

Bret Shandro and Pascal Haegeli

bshandro@sfu.ca

Received and published: 17 February 2018

The paper presents interesting approach of combination of "avalanche climates" and the snow avalanche hazard characteristics based on extensive database of the catalogued actual snow avalanches in western Canada. Results of similar "comprehensive" studies, going a bit further, were reported in past (i.e. Miagkov S.M. Kanaev L.A. (Eds.) Geografiia lavin [Geography of avalanches], Moscow: Izdatel'stvo Moskovskogo universiteta, 1992. 331p.). Also, there are several classifications of the snow avalanches produced in Russia, including the "genetic classification" of V.V.Dzuba presented in the book cited above, where the types of avalanches were related to meteorology and stratigraphy of snow cover, definitely related to the conclusions in the presented manuscript. But it was pub-

[Printer-friendly version](#)

[Discussion paper](#)



lished in Russian, used different climatic parameters and was mainly focused on the territory of USSR. Any comparison would probably have purely academic value.

We would like to thank Dr. Sokratov for the revealing perspective into the field of avalanche climate studies in Russia, however it is unfortunate that this work is not available in English.

The paper provides detailed review of the gradual development of the snow avalanche hazard assessment, not touching the risk evaluation side. In my view this should be expressed in the abstract and text more clearly. Risk is mentioned several times. Or, at least ideas on how different "avalanche problems" would affect the value of risk should be presented. There is such link in Statham et al. (2018) (the year in citations should be corrected), but that paper is only cited as a source of CMAH.

While the physical risk from avalanches is managed by continuously monitoring hazard conditions and choosing mitigation measures accordingly to reduce the risk to elements of value exposed to the existing hazard (e.g., infrastructure, buildings, people) to an acceptable level. Hence, risk only comes into play when the hazard interacts with elements of value and is therefore context dependent. Since our study examines the nature of avalanche hazard in Western Canada separate from the existing elements of value, risk is not included in our discussion. However, as pointed out by Dr. Sokratov, we reference publications (e.g., Statham et al., 2018) that describe the link of avalanche hazard to risk in detail. We believe that this is appropriate.

We would like to thank Dr. Sokratov for pointing out the error in our citations of Statham et al. (2018). We fixed the issue throughout the manuscript.

Strange not to see citation and links to "A seasonal snow cover classification system for local to global applications" by Sturm et al. in the "avalanche climate types" discussion. Evidently different "classes of snow covers" should affect

[Printer-friendly version](#)[Discussion paper](#)

the avalanche hazard.

We thank Dr. Sokratov for pointing out this shortcoming. Sturm et al. (1995) was an informative study for the Mock and Birkeland (2000) snow climate classification scheme. To address this issue, we added some text and a Sturm et al. (1995) citation at the beginning on page 3 on line 3:

Building on previous research, Mock and Birkeland (2000) introduced a classification algorithm that objectively classifies the local snow and avalanche climate of individual winter seasons ... The authors derived classification thresholds by analyzing the variabilities of the select input parameters (Sturm et al., 1995) at locations with established snow-climate classifications (Armstrong and Armstrong, 1986).

Very interesting is the analysis of the seasonal (is not it inter-annual?) variability in the prevalence of various snow avalanches hazard situations (Figure 6). Not quite clear what the anomalies in percent means in Figures (Tables?) 7 and 8 for the comparison of different years though. Same applies to Figure (Table?) 10.

Dr. Sokratov is correct to point out our word choice regarding winter to winter or inter-seasonal variability of typical avalanche prevalence values was imprecise. We therefore changed the caption of Fig. 6 (page 18) to, "Interseasonal prevalence of typical hazard situations." and adjusted the heading of section 5.2.1 to "Typical hazard situation prevalence: Interseasonal variability"

In response to the comments from the first reviewer (Dr. Mock) and reduce redundancy, we strengthened the prevalence bar charts (Fig. 6 and 9) by adding an additional bar representing mean prevalence values on the very left and we moved the prevalence anomaly figures (Figures 7, 8 & 10) into supplemental material. The intent of these anomaly figures it to explicitly highlight the interseasonal variability in prevalence values that are hard to visually extract directly from the bar charts. To clarify our intent, we

[Printer-friendly version](#)[Discussion paper](#)

have now added an additional sentence in the text before referring to the first anomaly figure (page 17, line 7):

... The seasonal anomaly values represent the percent difference between the seasonal prevalence value and the mean prevalence value for each of the thirteen avalanche hazard situations during the study period (Fig. S2)... .

To highlight positive and negative anomalies of temporal and spatial variability of hazard situation prevalence values we added red and blue shading to indicate positive and negative anomalies great than 5 percentage points respectively. Because of the coloring, we assumed that NHESD would be laying out these tables as figures. We are happy to oblige with whatever is most appropriate for NHESD.

Despite these notes the paper is really good and the presented approach can indeed be used for other regions, where such an extensive dataset on snow avalanches is available. And in my view this is the main limitation of the approach - suitable only for well-documented sites. My suggestion is to publish the paper as it is or with minor editions.

We would like to thank Dr. Sokratov for his positive assessment of our manuscript and his insightful suggestions.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-2>, 2018.

Printer-friendly version

Discussion paper

