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Natural Hazards and Earth System Sciences
Discussions

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

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Overall this is a very good paper, I suggest publication with some minor revisions. The authors have a unique avalanche dataset in W Canada to study their objectives on snow avalanche characteristic very closely. Methods are robust and results are very well described, including the SOM (Self-organizing maps), and figures in the most part are very good. Figures and tables a bit numerous, more so than the average manuscript, they are useful and present a lot of information, but I wonder how perhaps some can be condensed. It is hard for me to comment further at this point. Maybe more information can be added to Supplemental material online. Below are some comments.

1) The authors are correct on the top of p. 3 noting the little research on specifying the

nature of avalanches (loose wet slabs, etc.) as well as avalanche safety aspects within the context of broad climate types. The authors should note that Mock and Birkeland (2000) and some of their subsequent papers did note the role of short-term weather conditions within days to a few weeks (particularly as driven by synoptic patterns) on faceted crystals etc., so Mock and Birkeland did not simply just rely on the 3 main climate specifications. Thus assertions on interseasonal variability are not too surprising (bottom p. 26). The authors on p. 27, should strike out the limitation of Mock and Birkeland on ignoring the sequences of weather events, as they acknowledged the importance of studying shorter weather timescales.

2) The authors should note Fitzharris' older Rogers Pass work. Though old, it's the most comprehensive long-term avalanche record in W Canada dating back to the 19th century and has been studied extensively in its climate context.

3) p. 12, Table 1. On the SOM hazard situations (which is a very good research approach), I assume that avalanche control is not a factor in creating any of these?, and that the data set is truly unique with regards to avalanche control (which the authors describe later in the period that is usually a problem with avalanche data sets).

4) Section 5.2.2. Again I assume spatial variability of hazard situations are not impacted by aspects of avalanche control? This includes the high prevalence of deep persistence slabs in the alpine and treeline of the Central Rocky Mountain region.

5) Biggest thing missing in the analyses, and the logical next step would be to have a synoptic meteorological/climate component to relating with the appropriate hazard situations (ex. storm & wind slab events, spatial variability of hazard situations etc.), and if available, some snow pit data analyses. This should stress timeframes shorter than ENSO/seasonal, given the avalanche hazard situation findings. The large-scale meteorological analyses and forecast model simulations in some part, with recognition on non-weather factors etc. may provide additional forecast value to help in terms of probability, and perhaps explain some additional differences in seasons as presented

in Figures 6-8 (Seasonal prevalence of typical hazard situations). This can be stressed a bit strongly briefly in a few places in the paper.

6) Conclusion: the authors on p. 28 correctly point out the issues and difficulty in long term avalanche datasets and practices, and applications in the backcountry. They seem a bit too negative on long-term studies overall, and this should just be brief mention and be more constructive/complimentary, as long-term approaches are often studied differently with those issues in mind. That includes dendrochronological studies, which have very good value on probability of events, etc. Here, citing the Fitzharris studies show the value and perhaps an example of looking at things at a longer timeframe, and merging different interdisciplinary approaches.

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