Thanks you very much for asking me to review the paper. Just to make things clear, I was not involved in the first round of public discussion, but could see the referee comments in the open discussion.

The paper investigates the fluctuations of avalanche activity in Western Canada and its links to large-scale climate patterns. Main novelty of the article is to ground on avalanche problems series that result from avalanche forecasting bulletins. This is an excellent idea that has huge potential for many studies in the snow avalanche field. Also valuable is the chosen statistical methodology, namely a model-based assessment using a generalized linear model adapted to the data structure. Such an approach, even though it is not completely new, remains little used in the snow and avalanche field. Formally, the paper is written in an excellent English, probably far better than mine, so I won't comment any further language issues.

By contrast, the findings of the paper may be seen as a bit weak. We indeed already know that avalanche activity is weakly related to some atmospheric circulation patterns (see, e.g. Birkeland et al., 2001; Keylock 2005; Garcia Selles et al., 2010, Oeller et al., 2015) and even than this is true in Canada (McClung, 2013). The paper somewhat refine and reinforce this statement (different types of avalanches, different atmospheric indexes), but, from "outside the box", not that much, and it is not fully clear to which extent the results obtained may therefore be useful for a broad readership. This discussible added value to the state of the art regarding main drivers of avalanche activity is first due to the short period analyzed : you cannot expect to infer very significant patterns at a decadal time scale with ten years of data. Second there is also a more fundamental issue regarding how meaningful/useful it is to search for some linkages between avalanche activity and synoptic patterns rather than focusing on more direcrt local climate patterns. From a different perspective, the basics of the statistical method used, although clearly adapted to the data structure and amount, is very hard to follow and is not discussed in the paper.

All in all, there are some very interesting and innovative points in what the authors are proposing (avalanche problems as data source and model-based statistical analyses) but in my opinion they are insufficiently put forward in the paper which focus much more on results which do not appear as fully conclusive and/or "useful". I therefore highly recommend publication of the paper but after substantial reworking of the text, so as to more clearly present the overall benefit of the work done.

Avalanche problems from forecasting bulletins as data source

This is really the new, great, idea introduced by the paper. This provides regular daily information about the potential occurrence of different types of avalanches (referenced as avalanche problems). As a consequence, it has certainly lots of potential and will arguably be heavily used in other studies due to the recurrent lack of long and homogeneous data series in the field. A really see such an approach as a new way of providing avalanche data series usable for various scientific questions in all areas where regular forecasting bulletins are issued. I however see two issues in what the authors are proposing:

- The authors state at several points the superiority of their data with regard to real avalanche observations because of a higher homogeneity and because of being more informative regarding different types of avalanches. Again, I like the idea and the data, but a more "modest" posture would be preferable. As the authors themselves show, homogeneity is also an issue for such data (they have to distinguish data before/after 2012 in their analysis). Also, it exists high quality series of observed avalanches likely to provide insights of past changes, even of different avalanche types and over longer time periods (see, e.g. Eckert et al., 2013 for changes in avalanche flow regimes or Naaim et al., 2016 for changes in dry/dense flow type from observed data). Other types of data such as historical archives and indirect proxy data (e.g. Giacona et al., 2017) also provide interesting insights (e.g. Ballesteros-Canovas et al., 2018). Eventually, to which extent avalanche problems series reflect real avalanche activity remains somewhat unclear and is clearly a source of bias. Hence, rather than a better data source, it is one more, with different strength / weaknesses, which is already a lot, but could/should be discussed in a more comprehensive and fair way.

- The author process a ~10 year long data series. The statistical method chosen is consistent with this time frame (see below), but, anyhow, it is clearly short for investigating long term changes, even at the “decadal” time scale corresponding to synoptic patterns. So wouldn’t it be possible to generate longer avalanche problems series further back in the past even if operational forecasting did not exist at that time? I assume longer snow and weather records exist, and it could be possible to ask forecaster to issue some past bulletins on this basis, to use some machine learning techniques or even to combine both approaches to generate past series of daily avalanche problems. This would clearly make available information more insightful in terms of long-term changes.

Linkages to synoptic patterns

The authors propose a detailed analysis of the linkages between the different avalanche problems and various (in fact many) large scale atmospheric patterns. The study is done in a very rigorous way and results are deeply analysed, I have nothing to say about it except that : 1) it is long due to the high number of atmospheric indexes used and 2) it is not fully conclusive. The latter lack of strong result is not due to an inappropriate statistical methodology, but arguably to i) the short time frame covered ii) the possibly always weak link between large scale atmospheric patterns and local avalanche activity.

From the perspective of the last point my question is more “philosophical” about the interest of such an analysis. I know this is a topic that has some place in the snow avalanche field (e.g. Keylock 2005 and other references above) but I am always a bit unsure about the real added value in terms of short and long term forecasting. I mean, to interpret the results, it is always necessary to use local climate conditions as an intermediate. For example, l. 500 the authors interpret the positive linkage between wind slab avalanches and arctic oscillation by intense westerly flows. As a consequence, why is it so useful to highlight a weak link to AO? Is AO in the future easier to predict that regional snow and weather conditions which are clearly much more direct predictors of local avalanche activity? I am far from sure… Hence, why the focus of the study is on the link with synoptic patterns given the 10 year data series at hand seems to me unclear. This is all the more true that the authors use avalanche problems that do not necessarily reflect real activity. I would have expected first a detailed analysis of how avalanche problems series relate to real avalanche activity and local snow and weather conditions…

All in all, as I do not ask to change everything, I would suggest to reduce significantly the number of considered indexes, sticking on the most significant ones that may really bring “something” to our broad knowledge of the links between synoptic patterns and avalanche activity. At the same time, adding “intermediate” snow and weather data, as well as potentially local avalanche activity series (if these exist) would help really understand the results. Namely it should then be able to answer why there is a relation, strong or weak, between the avalanche activity which is observed locally and the synoptic pattern. Maybe there is no link because avalanche problems do not reflect real activity, or because local snow conditions are very poorly related to the synoptic pattern… In any case I would suggest that the authors elaborate on this points. This may be of broader interest for the readership than knowing if, on a 10 year time frame, AO is a bit better correlated to some avalanche problems in Canada than ENSO, for example.

Statistical modelling

Authors’ statistical strategy relies on the use of generalised linear models with a Beta likelihood adapted to model standardized proportions. This offers a suitable framework to investigate whether or not some effects (such as a synoptic pattern) is significant or not. I fully agree that this is a much better strategy given the 10 year data available than relaying only on correlation analyses, tests, p-values, etc. As stated earlier, such an approach is not fully new in the snow avalanche field (see., e.g. Eckert et al., 2010; Lavigne et al., 2015; 2017), but remains insufficiently used. However, I see two issues:

- The model is not presented in a formal, mathematical way. This makes very difficult to follow exactly what is done and especially how the stratification is built (what is grouped or not, etc.) and what are the different fixed and random effects that are considered. As this is quite frustrating, and should be changed,. At least the authors could include a devoted Appendix if they do not want any equations in the core of their text (but we are in a scientific journal, after all…)
- Model based statistics is the right way of processing small data samples, that's true (see e.g. Diggle 2007). You are gaining inferential power thanks to the model structure. In other words, effects/relations become more easily significant because of the modelling assumptions than with purely non-parametric data-based approaches. By contrast, you have to pay a price (nothing is granted for nothing) and that's the modelling assumptions. So this should be investigated/discussed somewhere, which is currently barely the case. Among potential issues: what about standard model fit to data diagnoses? And scores to evaluate whether or not other model structures than the one chosen would be more suitable? Eventually, I am wondering if the data content could not be more informatively used. As far as I understand, the 8 proportions are processed as independent quantities whether arguably some combinations are more likely than others. Could this be taken into account into the modelling? Same for space, could some kind of distance between regions be included (I assume close regions are more likely to behave in a similar way), etc.

Reference choices

The list is impressive. However it is strongly biased towards Canadian studies (or at least studies carried out in North America). This precludes discussing the approach and results in a broad context, and notably to highlight the main strength and weaknesses of what is proposed for a broad readership not especially interested in avalanche regime in western Canada. According to the reference earlier I would suggest the authors to insist more on existing knowledge on avalanche – synoptic patterns relations, avalanche data series and ong term changes, and theire processing with advanced model based statistical techniques.

Further formal aspects:

- Abstract seems way too long for an abstract
- Organization is a bit awkward. I assume that sect. 2 content could be easily moved to other sections (introduction, discussion).
- The paper is quite lengthy. Given that in my opinion, the main interest lies within the method (avalanche problems as data source and model-based statistical analyses) rather than within the results regarding linkages to synoptic patterns, it could certainly be significantly shortened without losing the key message, and focusing on the main novelties of broad interest.
- Figures 6-8 look a bit too much like direct outputs of a statistical software (namely R probably). I am wondering if something more "visual" and easy to read could be produced? Others figures are nice.

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