Responses to Referee #3

GENERAL COMMENT

We thank Referee #3 for their constructive review and helpful comments. We appreciate the encouraging comments about the potential of including avalanche problem information and the value of the model-based assessment approach we used in our study. Please see below for our detailed responses to specific comments and suggestions from Referee #3. Additions to the manuscript are included in our responses in quotes. In the revised manuscript, the edits are marked with the number of the Referee comment.

RESPONSES TO SPECIFIC COMMENTS

3.1 Value of analysis and focus of paper

Referee Comment:

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".

And later

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.

Author Response:

We appreciate the reviewer's comment and the opportunity to reflect on the value of our study.

In line with the reviewer's perspective, we believe that this manuscript contributes to the literature in two ways: a) by increasing our understanding of the effect of atmosphere-ocean oscillations on the nature of seasonal avalanche hazard in western Canada, and b) by presenting a new analysis approach (inclusion of avalanche problem information, multivariate, model-based analysis approach). However, despite the relatively short study period (which we openly acknowledge in several sections of the manuscript), we disagree with the reviewer that the results are 'weak' and do not warrant a detailed discussion. It is our opinion that the detailed description of the results is crucial for highlighting the validity and value of the analysis approach. Hence, we did not substantially shorten the manuscript or reduce the number of oscillation indices included in the analysis. The nature of avalanche hazard in western Canada shows distinct responses to the Arctic Oscillation and the combined Pacific-centered oscillations. In addition, we believe that combining ENSO, PDO and PNA into a single average index, openly acknowledges that our existing dataset does not allow us to properly isolate the effect of the individual atmosphere-ocean oscillations.

However, the reviewer's reflection on the added value for short- and long-term forecasting, made us realize that the practical motivation for this study (and the broader line of research in general) might not be clear. To address this issue, we expanded the last paragraph of the introduction to better explain our motivation. The revised paragraph reads as follows:

"The objective of the present study is to complement the existing research on the effect of large-scale atmosphere-ocean oscillations on avalanche hazard in western Canada by taking advantage of the avalanche problem information included in public avalanche bulletins that follow the conceptual model of avalanche hazard (Statham et al., 2018a). This approach links the analysis more closely to backcountry avalanche risk management and overcomes some of the shortcomings of previous studies. Even though linking avalanche hazard conditions to large-scale atmosphere ocean oscillations is unable to provide direct insight for operational, day-to-day avalanche safety decisions, a better understanding of these relationships has the potential to allow the avalanche safety community to take advantage of atmosphere-ocean oscillation predictions that are routinely provided by meteorological services to produce informative seasonal avalanche hazard forecasts. Being able to predict the general nature of seasonal avalanche conditions (e.g., there is a good chance that this winter will be dominated by a deep persistent avalanche problem) would help avalanche professionals and recreationists to develop meaningful risk management expectations for an upcoming season. As pointed out by LaChapelle (1980) and McClung (2002), avalanche forecasting is a dynamic and iterative process that resembles Bayesian updating where having a prior or hypothesis is critical."

While we previously discussed these ideas in the conclusion section, moving them to the introduction makes them more prominent. Furthermore, it should explain why including additional weather or snowpack observations in in the analysis would go against the objective to find a 'cheap' way to produce seasonal avalanche forecasts, which is distinctly different from producing models for offering short-term insight for operational avalanche safety operations or examining the effect of climate change on avalanche hazard.

We hope that the expanded introduction addresses the reviewer's concern adequately.

3.2 Strengths and weaknesses of avalanche problem dataset

Referee Comment:

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.

Author Response:

We appreciate this comment from the reviewer, and we agree that each dataset has its own strengths and weaknesses. In response to this comment, we have revised several sections of the manuscript to better reflect that our avalanche problem dataset and analysis simply provide a different perspective that complements the existing studies. See below for the text of the revised sections (marked with a comment "Reviewer comment 3.2" in the revised manuscript.

Introduction
"Furthermore, changes in avalanche risk mitigation practices along these transportation corridors can add noise to the avalanche activity record that make it more difficult to attribute the observed patterns to changes in winter weather (Bellaire et al., 2016; Sinickas et al., 2016; Jamieson et al., 2017)."
instead of
"Furthermore, the observed patterns in avalanche activity are difficult to conclusively attribute to changes in winter weather because the risk from avalanches along transportation corridors is tightly managed, which makes the available avalanche observation time series vulnerable to changes in avalanche control practices (Bellaire et al., 2016; Sinickas et al., 2016; Jamieson et al., 2017)."

Beginning of Section 2.2
"One of the challenges for examining the relationship between atmosphere-ocean oscillations and seasonal avalanche hazard is how to describe avalanche hazard in a meaningful way. While existing studies have primarily focused on the frequency of avalanches, the ratio between dry and wet avalanches, or the number of avalanche cycles, Atkins (2004) and Statham et al. (2018a) highlighted that the nature of avalanche hazard, its distribution in the terrain and evolution throughout the season are much more important for avalanche risk management than the frequency of avalanches alone."

instead of
"One of the challenges of existing studies examining the effect of atmosphere-ocean oscillations on avalanche hazard is the limited insight into the character of avalanche winters provided by the frequency of avalanches and the ratio between dry and wet avalanches. The nature and severity of avalanche problems, their distribution in the terrain and their evolution throughout the season are much more important for avalanche risk management than the frequency of avalanches alone (Atkins, 2004; Statham et al., 2018a)."

Discussion section
"While avalanche hazard assessment datasets are susceptible to changes in operational practices similar to avalanche observations time series, our knowledge of the change in forecasting practices at Avalanche Canada in 2012 allowed us to explicitly account for it by including an extra parameter in the Storm slab and Wind slab avalanche problem models." (new addition)

Conclusion section
"We believe that our approach complements and expands previous research in this area in several ways."
instead of
"We believe that our approach has several advantages over previous research in this area."

3.3. Expanding the avalanche problem dataset

Referee Comment:

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.

Author Response:

We completely concur with the reviewer that the limited length of our data set is one of the main limitations of our study, and we clearly state this in discussion section of the paper. While it would be nice to expand the dataset to earlier years, it is currently not possible. Operational avalanche forecasting is an evolutionary process that relies on a continuous integration of a wide range of avalanche safety observation (McClung, 2001). Fully assessing the nature of avalanche hazard of past winters for all of western Canada retrospectively would be an incredible amount of work that would likely not produce a reliable result.

Identifying and characterizing avalanche problems based on snow and weather records using machine learning algorithms is a topic of current research. See, e.g., Horton et al (2020) recently published in NHESS (https://nhess.copernicus.org/articles/20/3551/2020/), or the poster "How close are we to automated avalanche forecasting? Lessons from testing machine learning methods in Norway and Canada" presented by Horton, Müller, Haegeli and Engeset at the 2020 Virtual Snow Science Workshop (https://vssw2020.com/poster-submissions-2/). At this point, we are not in a position to reliably predicting avalanche problems from snow and weather observations that would contribute to this study in a meaningful way.

Hence, we did not make any modifications to the manuscript in response to this comment.

3.4 Presentation of model

Referee Comment:

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…)

Author Response:

We now include an Appendix where we give a formal expression of the beta mixed-effects model (in classical Laird-Ware formulation). We also include a few examples in R formula syntax which should help the reader to connect the computational model expression to the mathematical formulation. Please see appendix in revised manuscript for details. We also included a reference to the appendix in the main text of Section 3.3.

Please note that we also provide our full dataset and analysis code for readers interested in the full details of our analysis.

3.5 Description of model assumptions

Referee Comment:

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.

Author Response:

We appreciate the general approval of our analysis approach. With respect to the model assumptions, we assume that the reviewer's comment refers to regression diagnostics for checking those assumptions. As we are operating within a GLMM framework, simple (normal) residual checks do not work. We therefore used simulated quantile residuals (Dunn & Smyth, 1996) as implemented in the DHARMa package (Hartig, 2020) in R. Visual inspection of the resulting diagnostic plots (e.g., Q-Q-plot for uniformly distributed residuals) does not suggest any substantial model violations. We added the following new text in the last paragraph of Section 3.3.

"To assess violations in model assumptions, we simulated quantile residuals (Dunn and Smyth, 1996) as implemented in the DHARMa package (Hartig, 2020). Visual inspection of the resulting diagnostic plots (e.g., Q-Q-plot for uniformly distributed residuals) did not suggest any substantial model violations."

Regarding "scores to evaluate model structure" we assume that the reviewer is referring to model comparisons. For each avalanche problem we hypothesized two modeling scenarios: 1) a main effects model and 2) an interaction model where both AO and PO interact with the mountain range. Subsequently a likelihood-ratio test was applied to choose between these two hypothesized models. We did this for each response variable (Below treeline and Alpine/Treeline separately). This approach was already described in the manuscript. See highlighted section in updated manuscript with track changes.

We also describe in several places in the manuscript that our beta regression models are only able to capture monotonic relationships between the atmosphere-ocean oscillations and the prevalence of the different avalanche problem types (last paragraph of Section 4.1, third paragraph of Section 5). As highlighted at the end of the discussion section, we regard the dataset to be too small to include more sophisticated functional relationships in our analysis.

We also appreciate the reviewer's comments on alternative or expanded ways for examining our dataset. The reviewer correctly points out that some combinations of avalanche problem types are more likely than others. Prior to this study, our research team actually examined the same dataset of avalanche problem assessments using self-organizing maps to identify common pattern in avalanche problem combinations (Shandro and Haegeli, 2018: https://nhess.copernicus.org/articles/18/1141/2018/). However, since the derivation of these patterns is analytically rather involved and location specific, including it as an additional step in the present analysis makes the results less transparent, more difficult to interpret, and less reproducible. Hence, we believe that our approach of modelling each of the eight avalanche problems independently provides the most insightful and easy to interpret and reproduce contribution.

We agree with the reviewer that regions that are closer together will likely respond to the atmosphere-ocean oscillations more similarly. We included this aspect in our analysis by grouping for forecast areas into larger-scale regions. In our opinion, the current dataset is too small for employing a more sophisticated geospatial modelling approach would require the estimation of additional model parameters, and we are uncertain whether it would offer additional meaningful insight.

3.6 References biased towards North American studies

Referee Comment:

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 long term changes, and their processing with advanced model based statistical techniques.

Author Response:

Because the impacts of these atmospheric-ocean oscillations are very much location specific, and our description of the effect on western Canada is already quite long, we prefer not to expand our discussion to other geographic regions. However, the make the reader aware that similar studies have been conducted at other locations, we added the following sentence at the end of the second paragraph in the introduction:

"Similar studies have been conducted in other geographic regions including Iceland (Keylock, 2003) and the Pyrenes in Northern Spain (García-Sellés et al., 2010)."

While we appreciate the extensive list of references that the reviewer provided, we only included references that explicitly examine the effects of atmosphere-ocean oscillations on avalanche hazard and omitted other papers that focused on synoptic patterns or climate change to prevent the manuscript from becoming even longer.

3.7 Abstract

Referee Comment:

Abstract seems way too long for an abstract.

Author Response:

As suggested by the reviewer, we shortened the abstract. Please see the edited manuscript for the updated version of abstract.

3.8 Organization

Referee Comment:

Organization is a bit awkward. I assume that sect. 2 content could be easily moved to other sections (introduction, discussion).

Author Response:

While we appreciate this comment, we believe that it is in the realm of personal preferences for writing styles. We chose to have an explicit background section as it allows us to have a more concise introduction that present the reader with the objective of the study before getting into the fine details of some of the concepts. We believe that describing the relevant atmosphere-ocean oscillations affecting the winter weather in western Canada (2.1) and the concept of avalanche problems and its value for describing the nature of avalanche hazard (2.2) are critical pieces of information that the reader needs to understand before reading the method section of our manuscript. Hence, we do not believe that it would be meaningful to move content to the discussion section.

3.9 Length of paper

Referee Comment:

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.

Author Response:

We appreciate this comment of the reviewer, and in response, we have carefully reviewed the content of the paper. Overall, we believe that the detailed description of our results is necessary for highlighting the validity and value of our approach, and for linking our result to the existing literature in snow hydrology. Please also see our response to comment 3.1 for details on our perspective on the focus of the manuscript.

However, in response to this comment, we deleted the last paragraph of Section 2.2, which described the recent work of Shandro and Haegeli using avalanche problem information to describe the nature and variability of snow and avalanche climates in western Canada. While interesting, this information is not critical for understanding the present analysis.

3.10 Figures 6-8

Referee Comment:

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.

Author Response:

The results of our study are neatly summarized in Figure 4, which present differences in estimated marginal means for each atmosphere-ocean oscillation and region. The intent of Figures 5-8 was to provide the reader with a more detailed perspective on how these differences in marginal means relate to the prevalence observations and the regression models. Since differences in marginal means are an intuitive but fairly processed way to present the results, we believe that including these figures offers the transparency required in an academic publication.

While these figures were created in R, they were custom-built and not the standard output of an existing R package.

Hence, we did not make any changes in response to this comment.