

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-108

Karl W. Birkeland (Referee)

Referee comment on "Combining modelled snowpack stability with machine learning to predict avalanche activity" by Léo Viallon-Galinier et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-108-RC2>, 2022

In this paper the authors present a method using random forests to predict natural avalanches running to the valley bottom in the French Alps. Their methods appear to be solid, and the question they are trying to answer is important. In comparison to previous research, the novelty of their approach is that they make their predictions at the spatial scale of specific elevations and aspects. The paper is generally well-written and clear. I believe this research makes a valuable contribution, but I also feel there are issues that should be addressed prior to publication.

Here are a few of the major issues that I believe should be addressed:

- It would be helpful for the reader to better understand the spatial characteristics of the starting zones of the approximately 110 avalanche paths in the study area. Looking at Figure 1, it appears that most of the starting zones will have either a NW or a SE aspect. I am not sure about the distribution of the starting zone elevations. A Figure like Figure 2 (which shows the distribution of avalanche events by aspect and elevation) should be created for the avalanche path characteristics. In fact, it would be useful to pair this new Figure with Figure 2 so the reader could assess the effect of the avalanche path characteristics on the number of avalanches in each elevation/aspect zone.
- Along these same lines and again looking at Figure 1, I assume that the elevations and aspects of the avalanche starting zones are not evenly distributed in the 24 classes (three elevation and eight aspect categories). How does this affect the analyses? I understand that the authors would like to use the 24 elevation/aspect categories used in avalanche forecasts, but I wonder if it is appropriate to use all 24 categories for a dataset that appears to be unbalanced in the distribution of avalanche starting zone characteristics? How is this affecting their results?
- Another issue is the inclusion of both dry and wet snow avalanches in the same analysis. This was also pointed out by the other reviewer. Since we know that the avalanche release mechanisms for these two primary categories of avalanches are quite different, as are the meteorological factors that lead to instability, why are these included in the same analysis? Perhaps this is because both wet snow stability indices

and dry snow stability indices are included? Wouldn't it be better to split all the avalanches into "dry" and "wet" categories, and then proceed with the analysis on each of these two subsets of the data?

- The other reviewer also mentioned another issue I believe needs to be addressed. The dataset does not include all avalanches that occurred, but rather it consists predominantly of avalanches running to the valley floor. I assume these are almost all quite large avalanches. Can you provide a range of the size of the avalanches? Are they all Size 3 (on the Canadian or the U.S. destructive scale) or larger? Or perhaps size 4 or larger? What effect do the authors believe that this bias toward large avalanches has on their results?
- While the authors reference some of the more recent work on predicting avalanches with random forests, I feel like they might want to also reference some early work that attempts to better predict avalanche activity using the statistical techniques available at that time. These older papers had more the more modest goal of trying to predict avalanche days (without elevation/aspect of the starting zones), but they were a first step in this direction. This does not have to be a comprehensive review at all, but just a sentence or two with some references would be nice to see. Some older examples exist of researchers using discriminant analysis (examples: Bovis, 1977; Foehn and others, 1977), nearest neighbor techniques (example: Buser, 1983), and binary regression trees (example: Davis and others, 1992). Also, who was the first to use random forests for this type of work? Perhaps one of the authors who you already reference?
- Finally, one thing that perplexes me about this research is why new snowfall is rated so low in importance (Figure 4). This is completely different than prior research, which typically rated snowfall as the most important factor for dry avalanche release. Why do the authors believe this is the case? Is it because the "snow depth and variations" class is capturing this essential information? Or is it because of this information is captured (fully or partly) in some of the stability indices? Or is it the mixing of the dry and wet snow avalanches into one dataset? It might also be related to the fact that the dataset consists of only large avalanches. What do the authors think?

Despite the above comments, I believe this is valuable research and is deserving of publication once the authors address or respond to these issues.

I have also attached an annotated PDF, which includes corrections to some typographical errors, as well as further suggestions and suggested wording changes.

I hope the authors find my comments and suggestions useful.

Karl Birkeland

Some possible older references (the authors may have other/different older references they wish to cite):

Bovis, M.J. 1977. Statistical forecasting of snow avalanches, San Juan Mountains, Southern Colorado, U.S.A. *Journal of Glaciology* 18(78), 87-99.

Buser, O. 1983. Avalanche forecast with the method of nearest neighbors: An interactive approach. *Cold Regions Science and Technology* 8, 155-163.

Davis, R.E., K. Elder, and E. Bouzaglou. 1992. Applications of classification tree methodology to avalanche data management and forecasting. *Proceedings of the 1992 International Snow Science Workshop, Breckenridge, Colorado*, 123-133 (available at: <https://arc.lib.montana.edu/snow-science/item.php?id=1245>).

Foehn, P.M.B. and others. 1977. Evaluation and comparison of statistical and conventional methods of forecasting avalanche hazard. *Journal of Glaciology* 18(78), 375-387.

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2022-108/tc-2022-108-RC2-supplement.pdf>