Reply to reviewer comments Karl Birkeland

Frank Techel
Correspondence to: Frank Techel (techel@slf.ch)

Dear Karl

thank you for your detailed and very helpful review of our manuscript. We greatly appreciate the time and effort you put into reviewing our manuscript.
Please find below our responses (in blue) to your comments *(in italics)*.

This paper utilizes a data driven approach to look at the relative contributions of snowpack stability, the frequency distribution of snowpack stability, and avalanche size in assessing avalanche danger. The paper provides a novel analysis of a unique dataset to generate interesting results, and it will be a nice contribution to the literature. I believe the paper should be published after addressing several points.
First, the title could be worded more succinctly and less ambiguously. I might suggest something along the lines of "The importance of snowpack stability, the frequency distribution of snowpack stability, and avalanche size in assessing avalanche danger". However, the authors might have some other title they prefer. In particular I think they could omit "a data-driven approach" since that can be emphasized in the abstract and the text. Also, in the title and in several places in the paper they write «snowpack stability, its frequency distribution, and avalanche size:» . I personally find this to be a bit awkward and ambiguous with the use of the term "its". Even though it is slightly longer and involves more words, I think saying «snowpack stability, the frequency distribution of snowpack stability, and avalanche size» states what the authors are trying to say more clearly.
Thank you for this suggestion. We will change the title to «On the importance of snowpack stability, the frequency distribution of snowpack stability, and avalanche size in assessing the avalanche danger level» . Within the text, we will change accordingly where necessary.

Second, my main criticism of the paper relates to the conclusion by the authors that «avalanche size only has a rather minor influence on the danger level» (bottom of p. 23). Perhaps this is just from the author's choice of words, but in my opinion the data and Figures in the paper do not show a "rather minor influence". Instead, they show an influence that may be less than that of snow stability or frequency, but one that is still clearly evident. An example is in Figure 6 where no matter which letter you get from the combination of stability and frequency on the left side of the Figure, when you go to the right side of the Figure you can see that with all the letters you see an increase in the avalanche danger as the

largest avalanche size increases. This is also clearly shown in Figure 8, where going from left to right in the Figure we can see that the proportion of higher danger levels increases as the avalanche size increases. Another example of the influence of avalanche size can be seen in Figure 5. It is true, as the authors state in the Conclusions on p. 24, that "the largest avalanche size – used by itself – had comparably little discriminating power at 1-Low to 3-Considerable". However, while that might be strictly true for "the largest avalanche size", Figure 5 shows that the distribution of avalanche size – particularly of the largest avalanche (Figure 5b) – clearly does play into avalanche danger. The frequency distributions visibly tend toward larger avalanches at higher danger levels, with the proportion of size 3 and 4 avalanches increasing while the proportion of size 1 avalanches decreases. I would tend to disagree with the statement on p. 14, line 10-11, that Figure 5b shows "rather similar size distributions at 1-Low and 2-Moderate". Comparing the two, we can see a sizable decrease in size 1 avalanches and an almost doubling in the number of size 3 avalanches between Low and Moderate. Given the data presented in the paper, I would argue that the authors should better acknowledge that avalanche size does indeed have an influence on avalanche danger, and is not a "rather minor influence" (as stated on p. 23). I think they could still make an argument that snow stability, and the frequency of snow stability might well have a larger influence on avalanche danger, but avalanche size is also an important part of the avalanche danger assessment process. I would therefore encourage them to revisit various parts of the manuscript where avalanche size is discussed and better acknowledge the influence of size on avalanche danger.

Currently, we describe the findings shown in Fig. 5b with (p. 14 l. 10-14): «Considering the size of the largest reported avalanche per day and warning region showed again rather similar size distributions at 1-Low and 2-Moderate (Fig. 5b). The median largest avalanche per day and region was size 2 for 1-Low to 3-Considerable, except at 4-High with size 3. However, the proportion of days when size 1 avalanches were the largest observed avalanche decreased considerably with increasing danger level, while the proportion of days with at least one size 3 or size 4 avalanche increased monotonically. At 4-High, more than 75% of the days had at least one avalanche of size 3 or 4 recorded.» The statement «rather similar size distributions» is probably a bit ambiguous. Otherwise, this description corresponds to what is shown in the figure. We intend to rephrase to something like: "Considering the size of the largest reported avalanche per day and warning region showed that the largest avalanche per day and region was most frequently size 2 for 1-Low and 2-Moderate, a mix of size 2 and size 3 at 3-Considerable, and size 3 at 4-High (Fig. 5b). However, the proportion of days when size 1 avalanches were the largest observed avalanche decreased significantly with increasing danger level ($p < 0.001$), while the proportion of days with at least one size 3 or size 4 avalanche increased significantly ($p < 0.001$). At 4-High, more than 75% of the days had at least one avalanche of size 3 or 4 recorded." Regarding Figures 6 and 8: We agree, row-wise there is a slight increase in the proportion of higher danger levels with increasing avalanche size. However, if cells are ignored which have very few cases (in Fig. 6 cells with less than 1% of the data are highlighted, in Fig. 8 these are the cells with $N < 100$), then only rather minor differences can be seen. This contrasts to reading the figures column-wise. The danger level changes almost always from 1-Low in row E to 3-Considerable or 4-High in rows A and B. Hence, we believe the statement, which we make on p. 23 l. 33 that «avalanche size only has a rather minor influence on the danger level», is essentially true. However, we propose to rephrase along the line: «Even though there was a shift towards

more frequently observed larger avalanches with increasing danger level, the danger level was primarily influenced by the cell describing stability. [could add] Avalanche size (apparently) influences the danger level only if avalanches are small (size=1) or very large (size≥4).«- Please note that we intend to restructure the Results section, following the suggestions by referee Simon Horton. This means, that we will probably present the Swiss data first, and will therefore phrase the sentence again slightly different.

Thank you for the following suggestions and pointing out typographical errors, which we will address when revising the manuscript. Please find below comments on some of the points. However, we will provide a point-by-point reply together with our revised manuscript.

Below are some other suggestions and typographical errors that I believe the authors should address:

- *p. 1, line 2, delete "the"*
- *p. 1, line 4, remove the two commas*
- *p. 2, line 16, replace "weakest" with "the most unstable" because weakest could simply be a weak snowpack that has no slab and is therefore not unstable.* - We intend to use as an alternative term, probably something along the line "lowest stability" rather than "weak" or "the most unstable".
- *p. 2, line 23, what does the "(?)" refer to? Were the authors going to put a reference in there or ??*
- *p. 3, line 2, spell out EAWS completely the first time it is introduced in the text and then refer to it as EAWS afterwards.*
- *p. 3, line 7, remove the two commas and replace "work" with "works"*
- *p. 3, line 11, replace "but" with "and"*
- *p. 3, line 12, replace "And" with "and"*
- *p. 3, line 13, delete "does" and change "describe" to "describes"*
- *p. 3, line 24, delete the comma and remove the apostrophe from "biases"*
- *p. 3, line 25, delete "The target variable" and "we want to describe the three factors with"*
- *p. 4, line 17, would the authors like to include (Föhn, 1987) in addition to (Schweizer, 2002) to the RB reference?*
- *p. 5, line 1, replace "comparably" with "relatively"*

– *p. 6, line 1-3. It would be nice if the authors would explain why they removed the upper and lower 2.5% of the avalanche data. I am guessing they did this to filter out possible errors with the extremes or something along those lines? In any event, a single sentence explaining why this was done would be helpful.* - We will provide an explanation.

– *p. 7, line 5. The authors state that they are assuming that "different days with the same danger level exhibit similar stability distributions". I think they probably have to assume this to continue with their analyses. However, although I don't have any concrete data to support this, I feel like stability distributions can certainly vary between days that have the same danger level. This is somewhat built into the Conceptual Model of Avalanche Hazard by the inclusion of "uncertainty" and relates to how large an oval a person might put on the probability/size graph of the CMAH when selecting a danger level. It seems to me that the largest variations in stability distributions fall under "3 - Moderate" and "4 - Considerable" danger levels. For example, sometimes under 4 – Considerable you might have a distribution that is more spread out with the possibility of triggering a larger avalanche, while another time you might have a narrower spread of values, but the size of avalanche expected might be smaller. Both of these could have the same avalanche danger level, but the distribution of stability would vary. I don't think the authors have to make big changes to this paper, but I do think they should acknowledge that this assumption they are making might not always be valid.* - We agree, there is not one typical stability distribution for each danger level, but rather a typical range of distributions. This is essentially what we got when we applied the bootstrap-sampling approach (e.g. Figure C1c in the Appendix). At 3-Considerable, there were distributions with a proportion of 2% very poor and 25% good stability tests, but also some where these proportions were more than 20% very poor and 5% good. - We will rephrase accordingly.

– *p. 7, line 18, sentence is a bit awkward and confusing. I would change it to read: "Since nature is not as discrete as the danger levels suggest, we wanted both some overlap between our sampled stability distributions and a reasonably high resolution of our statistic."*

– *p. 9, line 12, replace "maximising" with "maximizing"*

– *p. 11, Figure 3. This is an interesting and important Figure. One limitation that is noted in the text and also in the figure is the very small N for "4-High" (approximately two orders of magnitude smaller than for 2-Moderate or 3-Considerable). To further emphasize this, the authors could consider stating something related to this in the Figure caption, possibly something like "Note the small N for 4-High for both tests", or, even better, you could write "Note the N for 4-High is small and is approximately two orders of magnitude less than the N for 2-Moderate or 3-Considerable".* - We will add a note in this regard.

– *p. 12, line 11, delete the first "of" in the line.*

– *p. 14, line 19, delete "It is of"*

- *p. 19, line 4. I have seen this under representation of smaller avalanches in most datasets related to ski area snow safety staff in the United States. This isn't written down in too many places, but we do discuss this somewhat in (Birkeland and Landry, 2002) (Power-laws and snow avalanches. Geophysical Research Letters 29(11), 49-1 to 49-3).* Thank you for pointing this out. This supports the findings shown in 5a, and the literature that we have cited in the Discussion section (p. 18 bottom and p. 19).
- *p. 19, line 6. Replace "As" with "Since" and insert "instead" between "focused" and "on".*
- *p. 19, line 8, delete comma*
- *p. 19, line 9, replace "weak" with "unstable". I believe the authors are talking about an "unstable" snowpack here and not necessarily one that is just structurally weak, correct?*
- *p. 20, line 23 and 25 (and probably elsewhere in the manuscript), replace "in prep." with "under review".*
- *p. 21, line 23, delete "It is of" and replace "was" with "were"*
- *p. 21, line 26, replace "," with "." prior to "For instance,"*
- *p. 21, line 28, replace "Schweizer et al. (2003) s" with "Schweizer et al.'s (2003)"*
- *p. 22, line 25 and 27. The authors refer to the correlations being "strong" or "moderate". What do you mean by this? Are they statistically significant or not? You might want to state whether they are significant and list a p-value. When I refer back to Section 4.1.2 as is suggested on line 27, I believe the authors are referring to p. 12, line 5-8. Is this correct? Here it states that – even with an N = 10 - the correlation is highly significant ($p < 0.001$).* - Yes, these terms refer to the way Spearman rank-order correlations are interpreted. Both were indeed highly significant. - We will rephrase.
- *p. 23, line 5. What does the "(?)" refer to? Are the authors planning on adding a reference here?*
- *p. 30, delete "and tables" from the title of Appendix 2 since this appendix has only figures.*
- *p. 31, in the caption for Figure B1, replace "Fig.s" with "Figs."*
- *p. 34, Figure E1, for the top right part of the Figure (all avalanches for Switzerland), add "(SWI)" after "all avalanches" to be consistent with the other headers. Also, add the percent number above the bar for size 1 avalanches under Low to match the other graphs in this Figure.*

Frank Techel, on behalf of all co-authors

References

Birkeland, K. and Landry, C.: Power-laws and snow avalanches, Geophysical Research Letters, 29, doi:10.1029/2001GL014623, 2002.

Föhn, P.: The rutschblock as a practical tool for slope stability evaluation, IAHS Publ., 162, 223–228, 1987.

Schweizer, J.: The Rutschblock test - procedure and application in Switzerland, The Avalanche Review, 20, 14–15, 2002.