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Discussions

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Interactive comment on "Avalanche danger level characteristics from field observations of snow instability" *by* Jürg Schweizer et al.

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I selected "Major Revisions" for this paper simply because some of my comments suggest that the paper may benefit from re-analyzing some of the data. However, I do not think that such a re-analysis should be overly difficult, and so my review probably falls somewhere between "major revisions" and "minor revisions".

This research aims "to characterize the avalanche danger levels based on expert field observations of snow instability." This is an important goal and is something that would be beneficial for avalanche forecasters and for a better understanding avalanche danger. The authors utilize an interesting, unique, and high-quality data set. While I believe the authors have produced an interesting paper, I believe it would benefit from some

changes and clarifications. I have four major comments:

1. First, when I was initially and quickly reading the paper and looking at the title, I assumed that the "avalanche danger" referred to in the title was the regional avalanche danger rating. However, this is not the case. Rather the "avalanche danger rating" is really the local nowcast provided by the observer. As pointed out in the paper, there is a scale mismatch between a local rating for a particular area and a regional avalanche danger rating. In addition, the authors point out that they are utilizing a local nowcast in their abstract. Despite this, I believe the authors should more clearly define the differences between these two ratings and if they decide to utilize the local nowcast avalanche danger then that should be specified in the title.

2. Second, along the lines of my first comment, the authors acknowledge that there is circular reasoning in their data since the observers are making snowpack and avalanche observations and are also assigning the local avalanche danger. Undoubt- edly the observers are taking their observations into account when they are assigning the local avalanche danger. I believe this is somewhat problematic. In this scenario the authors may actually be testing what snowpack observations the observers hap- pen to associate with a particular local avalanche danger rating rather than the more general question of which snowpack observations are associated with a danger level. I am wondering why the authors didn't simply compare the local observations to the re- gional avalanche danger assigned for that region for that day? That way there would be independence between the snowpack observer and the assessment of the avalanche danger, and the results would reflect the frequency of making these observations for a given regional danger level rating. I would suggest either utilizing the regional danger ratings or providing a solid rationale for not using them in the paper.

3. Third, this paper utilizes a unique dataset of snow profiles and observations from Switzerland. Approximately 95% of the data are from the region of Davos. Later the authors explain that some of their results, such as the predominance of persistent weak layers, may be because so much of their data are from the Davos area. I believe the

paper would benefit from using only those snow profiles and observations from around Davos, rather than a highly unbalanced dataset consisting of almost all profiles from Davos and then 5% from other areas. This would still retain 95% of the data but would remove some of the variability introduced by the other 5% of the data.

4. My fourth comment relates back to my third. The authors spend considerable effort (and content in their paper) characterizing the snow profiles. This provides some interesting results that I believe should be retained, but it is outside the primary stated goal of the paper. As stated above, I believe this analysis would have more meaning if the data were restricted to just the 95% of data from the region of Davos. Then the snowpack characterization part of the paper can provide a characterization of the Davos area snowpack rather than "mostly" the Davos area snowpack with 5% of the profiles and observations from other areas. In addition, this part of the paper should be better highlighted in the abstract and perhaps the title as well since nearly as much attention is paid to this snowpack characterization as is paid to the relationship of the snow profiles and observations to the danger levels. I think that this "characterization of a snowpack" in a region is quite valuable and will set a baseline for future work which could compare this characterization against the characterization of the snowpack in other regions or other countries.

While I do have some substantial comments that I believe the authors should address, I do think that this is important work and that it should be published following revisions.

In addition to the above comments, I have some more minor comments and suggested typographical corrections:

Line 29: Delete "at times"

Line 40: Delete "were" and replace with "have been"

Line 68: Replace "inexistent" with "nonexistent"

Line 84: Can you provide more specifics about how you defined "an experienced ob-

server"?

Line 97: After reading this paragraph, I am still not certain how the failure layer and adjacent layers are defined. I understand how the authors come up with the failure interface, but how do they necessarily define the failure layer? Was this done manually by the authors? And, with the adjacent layer, was this typically just the layer adjacent to the failure interface that was not the failure layer? I think this might be less confusing if the authors talked about the failure interface, and then the layer above that interface and the layer below that interface. They could then also quantify how often the "failure layer" is below or above the layer interface.

Line 115 – It seems like assigning 3+ to Considerable is arbitrary. Why not apply High here since 2+ is also assigned to Considerable?

Line 199-200 and line 525 – I don't think that a decrease in the whole block release of RBs with increasing RB number is necessarily related to a decrease in crack propagation propensity. This may have more to do with the increased damage to the slab caused by harder jumping on the RB that causes partial block releases. Given the complexities involved, I don't think the authors can draw such a definitive conclusion from their data. While I don't have a good dataset to either confirm or refute this conclusion, I have anecdotally seen seasons where PSTs consistently propagate to end – indicating the propensity for crack propagation – for a long time after other tests have indicated that failure initiation is far less likely.

Line 528: I assume the "avalanche danger level" in the sentence on this line is the local avalanche danger? As stated in one of my primary comments, it would be helpful to make sure to careful differentiate between the local avalanche danger rating and the "avalanche danger rating". I always assume this latter term is associated with the regional avalanche danger rating.

Line 529: Along the lines of my comment above, the authors state that the local avalanche danger rating agrees with 70% of the regional danger ratings. It would

be interesting to more thoroughly compare how those two ratings at the two different scales differ.

Line 454: Delete "and" and replace with "or"

Karl Birkeland